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PROCEEDINGS

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OF

THE AMERICAN ASSOCIATION

ERRATUM.

Page 91 (eleventh line from bottom), change "1.4328."

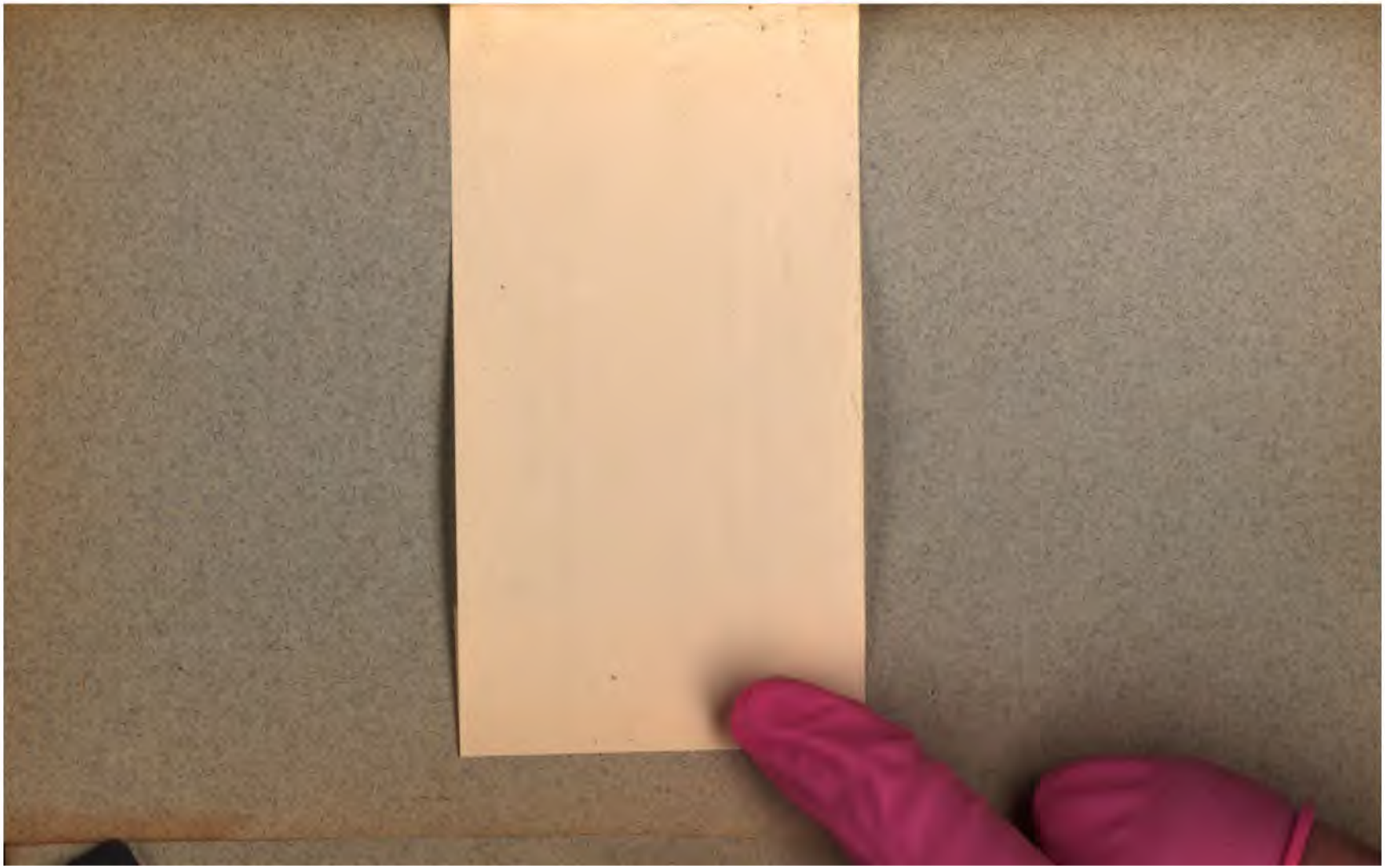
ERRATUM SLIP TO BE INSERTED IN

Add to list of officers of Section I on page 491 "retary, William H. Hale."

BOSTON, MASS.

AUGUST, 1898.

SALEM:
PUBLISHED BY THE PERMANENT SECRETARY.
DECEMBER, 1898.



PROCEEDINGS

79304

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE

FORTY-SEVENTH MEETING

AND

FIFTIETH ANNIVERSARY.

HELD AT

BOSTON, MASS.

AUGUST, 1898.

SALEM:
PUBLISHED BY THE PERMANENT SECRETARY.
DECEMBER, 1898.

EDITED BY

L. O. HOWARD, *Permanent Secretary*,

WASHINGTON, D. C.

Salem Press:
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1898.

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For a full list of Members of the several Local Committees see General and Daily Programmes of the Meeting.

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R. S. WOODWARD, Columbia University.

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From the Association at Large. — To hold over until successors are elected. A fellow from each section.— E. C. PICKERING of Cambridge (A); H. S. CARHART of Ann Arbor (B); F. W. CLARKE of Washington (C); J. B. JOHNSON of St. Louis (D); I. C. WHITE of Morgantown (E); W. H. DALL of Washington (F); W. W. ROWLEE of Ithaca (G); ALICE C. FLETCHER of Cambridge (H); W. R. LAZENBY of Columbus (I).

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A. S. PACKARD.

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[1] All Committees are expected to present their reports to the COUNCIL not later than the third day of the meeting. Committees sending their reports to the Permanent Secretary one month before a meeting can have them printed for use at the meeting.

MEETINGS AND OFFICERS OF THE ASSOCIATION OF AMERICAN GEOLOGISTS AND NATURALISTS.

MEETING.	DATE.	PLACE.	CHAIRMAN.	SECRETARY.	ASSIS'T SEC'Y.	TREASURER.
1st	April 2, 1840,	Philadelphia.	Edward Hitchcock,*	L. C. Beck,*		
2d	April 5, 1841,	Philadelphia.	Benjamin Silliman,*	L. C. Beck.*	B. Silliman, jr.,* C. B. Trego,*	
3d	April 25, 1842,	Boston,	S. G. Morton,*	C. T. Jackson,*	J. D. Whitney,* M. B. Williams,*	
4th	April 26, 1843,	Albany.	Henry D. Rogers,*	B. Silliman, jr.,*		John Locke.*
5th	May 8, 1844,	Washington.	John Locke,*	B. Silliman, jr.,* O. P. Hubbard,		Douglas Houghton.*
6th	April 30, 1845,	New Haven.	Wm. B. Rogers,*	B. Silliman, jr.,* J. Lawrence Smith,*		Douglas Houghton.*
7th	Sept. 2, 1846,	New York.	C. T. Jackson.*	B. Silliman, jr.,*		E. C. Herrick.*
8th	Sept. 20, 1847,	Boston.	Wm. B. Rogers,†*	Jeffries Wyman,*		B. Silliman, jr.*

* Deceased.

† Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

MEETINGS.	PLACE.	DATE.	MEMBERS IN ATTENDANCE.	NUMBER OF MEMBERS.
1.	Philadelphia	Sept. 20, 1848	?	461
2.	Cambridge	Aug. 14, 1849	?	540
3.	Charleston	Mar. 12, 1850	?	622
4.	New Haven	Aug. 19, 1850	?	704
5.	Cincinnati	May 5, 1851	87	800
6.	Albany	Aug. 19, 1851	194	769
7.	Cleveland	July 28, 1853	?	940
8.	Washington	April 26, 1854	168	1004
9.	Providence	Aug. 15, 1855	166	605
10.	2nd Albany	Aug. 20, 1856	381	722
11.	Montreal	Aug. 12, 1857	351	946
12.	Baltimore	April 28, 1858	190	962
13.	Springfield	Aug. 3, 1859	190	862
14.	Newport	Aug. 1, 1860	135	644
15.	Buffalo	Aug. 15, 1866	79	687
16.	Burlington	Aug. 21, 1867	73	415
17.	Chicago	Aug. 5, 1868	259	686
18.	Salem	Aug. 18, 1869	244	511
19.	Troy	Aug. 17, 1870	188	536
20.	Indianapolis	Aug. 16, 1871	196	668
21.	Dubuque	Aug. 15, 1872	164	610
22.	Portland	Aug. 20, 1873	195	670
23.	Hartford	Aug. 12, 1874	224	722
24.	Detroit	Aug. 11, 1875	165	807
25.	2nd Buffalo	Aug. 23, 1876	215	867
26.	Nashville	Aug. 29, 1877	173	953
27.	St. Louis	Aug. 21, 1878	134	962
28.	Saratoga	Aug. 27, 1879	256	1030
29.	Boston	Aug. 25, 1880	997	1555
30.	2nd Cincinnati	Aug. 17, 1881	500	1699
31.	2nd Montreal	Aug. 23, 1882	937	1922
32.	Minneapolis	Aug. 15, 1883	328	2033
33.	2nd Philadelphia	Sept. 3, 1884	1261*	1981
34.	Ann Arbor	Aug. 26, 1885	364	1956
35.	3d Buffalo	Aug. 18, 1886	445	1886
36.	New York	Aug. 10, 1887	729	1956
37.	2nd Cleveland	Aug. 14, 1888	342	1964
38.	Toronto	Aug. 26, 1889	424	1952
39.	2d Indianapolis	Aug. 19, 1890	364	1944
40.	2d Washington	Aug. 19, 1891	653†	2054
41.	Rochester	Aug. 17, 1892	456	2037
42.	Madison	Aug. 17, 1893	290	1939
43.	Brooklyn	Aug. 15, 1894	488	1802
44.	2d Springfield	Aug. 28, 1895	368	1913
45.	4th Buffalo	Aug. 24, 1896	333	1890
46.	2d Detroit	Aug. 9, 1897	283‡	1783
47.	2d Boston	Aug. 22, 1898	903	1729
48.	Columbus	Aug. 19, 1899	——	——

* Including 303 members of the British Association and 9 other foreign guests.

† Including 24 Foreign Honorary members for the meeting.

‡ Including 15 Foreign Honorary members and associates for the meeting.

OFFICERS OF THE MEETINGS OF THE ASSOCIATION.

[The number before the name is that of the meeting; the year of the meeting follows the name; the asterisk after a name indicates that the member is deceased.]

PRESIDENTS.

1. { WM. B. ROGERS,* 1848. / W. C. REDFIELD,* 1848.
2. JOSEPH HENRY,* 1849.
3, 4, 5. A. D. BACHE,* March meeting, 1850, in absence of JOSEPH HENRY.* August meeting, 1850. May meeting, 1851.
6. LOUIS AGASSIZ,* August meeting, 1851.
(No meeting in 1852).
7. BENJAMIN PIERCE,* 1853.
8. JAMES D. DANA,* 1854.
9. JOHN TORREY,* 1855.
10. JAMES HALL,* 1856.
11, 12. ALEXIS CASWELL,* 1857, in place of J. W. BAILEY,* deceased. 1858, in absence of JEFFRIES WYMAN.*
13. STEPHEN ALEXANDER,* 1859.
14. ISAAC LEA,* 1860.
(No meetings for 1861-65).
15. F. A. P. BARNARD,* 1866.
16. J. S. NEWBERRY,* 1867.
17. B. A. GOULD,* 1868.
18. J. W. FOSTER,* 1869.
19. T. STERRY HUNT,* 1870, in the absence of WM. CHAUVENET.*
20. ASA GRAY,* 1871.
21. J. LAWRENCE SMITH,* 1872.
22. JOSEPH LOVERING,* 1873.
23. J. L. LECONTE,* 1874.
24. J. E. HILGARD,* 1875.
25. WILLIAM B. ROGERS,* 1876.
26. SIMON NEWCOMB, 1877.
27. O. C. MARSH, 1878.
28. G. F. BARKER, 1879.
29. LEWIS H. MORGAN,* 1880.
30. G. J. BRUSH, 1881.
31. J. W. DAWSON, 1882.
32. C. A. YOUNG, 1883.
33. J. P. LESLEY, 1884.
34. H. A. NEWTON,* 1885.
35. EDWARD S. MORSE, 1886.
36. S. P. LANGLEY, 1887.
37. J. W. POWELL, 1888.
38. T. C. MENDENHALL, 1889.
39. G. LINCOLN GOODALE, 1890.
40. ALBERT B PRESCOTT, 1891.
41. JOSEPH LECONTE, 1892.
42. WILLIAM HARKNESS, 1893.
43. DANIEL G. BRINTON, 1894.
44. E. W. MORLEY, 1895.
45. { EDWARD D. COPE,* 1896. / THEODORE GILL, as senior vice president acted after the death of Prof. COPE.
46. WOLCOTT GIBBS, 1897, absent.
W J MCGEE, Acting President.
47. F. W. PUTNAM, 1898.
48. EDWARD ORTON, 1899.

VICE PRESIDENTS.

There were no Vice Presidents until the 11th meeting when there was a single Vice President for each meeting. At the 24th meeting the Association met in Sections A and B, each presided over by a Vice President. At the 31st meeting nine sections were organized, each with a Vice President as its presiding officer. In 1886, Section G (Microscopy) was given up. In 1892, Section F was divided into F, Zoology; G, Botany.

1857–1874.

11. ALEXIS CASWELL,* 1857, acted as President.
12. JOHN E. HOLBROOK,* 1858, not present.
13. EDWARD HITCHCOCK,* 1859.
14. B. A. GOULD,* 1860.
15. A. A. GOULD,*1866, in absence of R. W. GIBBES.
16. WOLCOTT GIBBS, 1867.
17. CHARLES WHITTLESEY,* 1868.
18. OGDEN N. ROOD, 1869.
19. T. STERRY HUNT,* 1870, acted as President.
20. G. F. BARKER, 1871.
21. ALEXANDER WINCHELL,* 1872.
22. A. H. WORTHEN,* 1873, not present.
23. C. S. LYMAN,* 1874.

1875–1881.

Section A.—Mathematics, Physics and Chemistry.

24. H. A. NEWTON,* 1875.
25. C. A. YOUNG, 1876.
26. R. H. THURSTON, 1877, in the absence of E. C. PICKERING.
27. R. H. THURSTON, 1878.
28. S. P. LANGLEY, 1879.
29. ASAPH HALL, 1880.
30. WILLIAM HARKNESS, 1881, in the absence of A. M. MAYER.*

Section B.—Natural History.

24. J. W. DAWSON, 1875.
25. EDWARD S. MORSE, 1876.
26. O. C. MARSH, 1877.
27. AUG. R. GROTE, 1878.
28. J. W. POWELL, 1879.
29. ALEXANDER AGASSIZ, 1880.
30. EDWARD T. COX, 1881, in the absence of GEORGE ENGELMANN.*

CHAIRMEN OF SUBSECTIONS, 1875–1881.

Subsection of Chemistry.

24. S. W. JOHNSON, 1875.
25. G. F. BARKER, 1876.
26. N. T. LUPTON,* 1877.
27. F. W. CLARKE, 1878.
28. F. W. CLARKE, 1879, in the absence of IRA REMSEN.
29. J. M. ORDWAY, 1880.
30. G. C. CALDWELL, 1881, in the absence of W. R. NICHOLS.*

Subsection of Microscopy.

25. R. H. WARD, 1876.
26. R. H. WARD, 1877.
27. R. H. WARD, 1878, in the absence of G. S. BLACKIE.*
28. E. W. MORLEY, 1879.
29. S. A. LATTIMORE, 1880.
30. A. B. HERVEY, 1881.

Subsection of Anthropology.

24. LEWIS H. MORGAN,* 1875.
25. LEWIS H. MORGAN,* 1876.
26. DANIEL WILSON,* 1877, not present.
27. United with Section B.
28. DANIEL WILSON,* 1879.
29. J. W. POWELL, 1880.
30. GARRICK MALLERY,* 1881.

Subsection of Entomology.

30. J. G. MORRIS,* 1881.

VICE PRESIDENTS OF SECTIONS, 1882–

Section A.—Mathematics and Astronomy.

31. W. A. ROGERS,* 1882, in the absence of WM. HARKNESS.
32. W. A. ROGERS, 1883.
33. H. T. EDDY, 1884.
34. WM. HARKNESS, 1885, in the absence of J. M. VAN VLECK.
35. J. W. GIBBS, 1886.
36. J. R. EASTMAN, 1887, in place of W. FERREL,* resigned.
37. OSMOND STONE, 1888.
38. R. S. WOODWARD, 1889.
39. S. C. CHANDLER, 1890.
40. E. W. HYDE, 1891.
41. J. R. EASTMAN, 1892.
42. C. L. DOOLITTLE, 1893.
43. [illegible] C. CLOSSON, 1894.
[illegible] FISCHER PEIRCE, 1894.
44. FISCHER PEIRCE, 1895, in place of F. R. HASSLER, resigned.
45. ALEX. MACFARLANE, 1896, in place of WM. F. STOCKY, resigned.
46. W. W. BEMAN, 1897.
47. E. E. BARNARD, 1898.
48. ALEXANDER MACFARLANE, 1899.

Section B.—Physics.

49. [illegible]
50. [illegible]
51. [illegible]
52. [illegible]
53. [illegible]
54. [illegible]
55. [illegible]
56. [illegible]
57. [illegible]
58. [illegible]
59. [illegible]
60. [illegible]
61. [illegible]
62. [illegible]
63. [illegible]
64. [illegible]
65. [illegible]
66. [illegible]
67. [illegible]
68. [illegible]

Section C.—Chemistry.

31. H. C. BOLTON, 1882.
32. E. W. MORLEY, 1883.
33. J. W. LANGLEY, 1884.
34. N. T. LUPTON,* 1885, in absence of W. R. NICHOLS.*
35. H. W. WILEY, 1886.
36. A. B. PRESCOTT, 1887.
37. C. E. MUNROE, 1888.
38. W. L. DUDLEY, 1889.
39. R. B. WARDER, 1890.
40. R. C. KEDZIE, 1891.
41. ALFRED SPRINGER, 1892.
42. EDWARD HART, 1893.
43. T. H. NORTON, 1894.
44. WM. MCMURTRIE, 1895.
45. W. A. NOYES, 1896.
46. W. P. MASON, 1897.
47. EDGAR F. SMITH, 1898.
48. F. P. VENABLE, 1899.

Section D.—Mechanical Science and Engineering.

51. W. P. TROWBRIDGE,* 1882.
52. [illegible] 1883, in absence [illegible].
53. [illegible]
54. [illegible]
55. [illegible]
56. [illegible]
57. [illegible]
58. [illegible]
59. [illegible]
60. [illegible]
61. [illegible]
62. [illegible]
63. [illegible]
64. [illegible]
65. [illegible]
66. [illegible]
67. [illegible]
68. [illegible]

VICE PRESIDENTS OF SECTIONS, CONTINUED.

Section E.—Geology and Geography.

31. E. T. COX, 1882.
32. C. H. HITCHCOCK, 1883.
33. N. H. WINCHELL, 1884.
34. EDWARD ORTON, 1885.
35. T. C. CHAMBERLIN, 1886.
36. G. K. GILBERT, 1887.
37. GEORGE H. COOK,* 1888.
38. CHARLES A. WHITE, 1889.
39. JOHN C. BRANNER, 1890.
40. J. J. STEVENSON, 1891.
41. H. S. WILLIAMS, 1892.
42. CHARLES D. WALCOTT, 1893.
43. SAMUEL CALVIN, 1894.
44. JED. HOTCHKISS, 1895.
45. B. K. EMERSON, 1896.
46. I. C. WHITE, 1897. E. W. CLAYPOLE, 1897.
47. H. L. FAIRCHILD, 1898.
48. J. F. WHITEAVES, 1899.

Section F.—Biology, 1882–92.

31. W. H. DALL, 1882.
32. W. J. BEAL, 1883.
33. E. D. COPE,* 1884.
34. T. J. BURRILL, 1885, in the absence of B. G. WILDER.
35. H. P. BOWDITCH, 1886.
36. W. G. FARLOW, 1887.
37. C. V. RILEY,* 1888.
38. GEORGE L. GOODALE, 1889.
39. C. S. MINOT, 1890.
40. J. M. COULTER, 1891.
41. S. H. GAGE, 1892.

Section F.— Zoölogy.

42. HENRY F. OSBORN, 1893.
43. J. A. LINTNER, 1894, in place of S. H. SCUDDER, resigned.
44. L. O. HOWARD, 1895, in place of D. S. JORDAN, resigned.
45. THEO. GILL, 1896.
46. L. O. HOWARD, 1897, in place of G. BROWN GOODE,* deceased.
47. A. S. PACKARD, 1898.
48. S. H. GAGE, 1899.

Section G.—Microscopy, 1882–85.

31. A. H. TUTTLE, 1882.
32. J. D. COX, 1883.
33. T. G. WORMLEY, 1884.
34. S. H. GAGE, 1885. (Section united with F in 1886.)

Section G.—Botany.

42. CHARLES E. BESSEY, 1893.
43. L. M. UNDERWOOD, 1894. C. E. BESSEY, 1894.
44. J. C. ARTHUR, 1895.
45. N. L. BRITTON, 1896.
46. G. F. ATKINSON, 1897.
47. W. G. FARLOW, 1898.
48. C. R. BARNES, 1899.

Section H.—Anthropology.

31. ALEXANDER WINCHELL,* 1882.
32. OTIS T. MASON, 1883.
33. EDWARD S. MORSE, 1884.
34. J. OWEN DORSEY,* 1885, in absence of W. H. DALL.
35. HORATIO HALE,* 1886.
36. D. G. BRINTON, 1887.
37. CHARLES C. ABBOTT, 1888.
38. GARRICK MALLERY,* 1889.
39. FRANZ BAKER, 1890.
40. JOSEPH JASTROW, 1891.
41. W. H. HOLMES, 1892.
42. J. OWEN DORSEY,* 1893.
43. FRANK BOAS, 1894.
44. F. H. CUSHING, 1895.
45. ALICE C. FLETCHER, 1896.
46. W J MCGEE, 1897.
47. J. MCK. CATTELL, 1898.
48. THOMAS WILSON, 1899.

Section I.— Social and Economic Science.

31. E. B. ELLIOTT,* 1882.
32. FRANKLIN B. HOUGH,* 1883.
33. JOHN EATON,* 1884.
34. EDWARD ATKINSON, 1885.
35. JOSEPH CUMMINGS,* 1886.
36. H. E. ALVORD, 1887.
37. CHARLES W. SMILEY, 1888.
38. CHARLES S. HILL, 1889.
39. J. RICHARDS DODGE, 1890.
40. EDMUND J. JAMES, 1891.
41. L. F. WARD, 1892, in place of S. D. HORTON,* resigned.
42. WILLIAM H. BREWER, 1893.
43. HENRY FARQUHAR, 1894.
44. B. E. FERNOW, 1895.
45. W. L. LAZENBY, 1896.
46. R. T. COLBURN, 1897.
47. ARCHIBALD BLUE, 1898.
48. MARCUS BENJAMIN, 1899.

SECRETARIES.

General Secretaries, 1848–

1. WALTER R. JOHNSON,* 1848.
2. E. N. HORSFORD,* 1849, in absence of JEFFRIES WYMAN.*
3. L. R. GIBBS, 1850, in absence of E. C. HERRICK.*
4. E. C. HERRICK,* 1850.
5. WILLIAM B. ROGERS,* 1851, in absence of E. C. HERRICK.*
6. WILLIAM B. ROGERS,* 1851.
7. S. ST. JOHN,* 1853, in absence of J. D. DANA.*
8. J. LAWRENCE SMITH,* 1854.
9. WOLCOTT GIBBS, 1855.
10. B. A. GOULD,* 1856.
11. JOHN LECONTE,* 1857.
12. W. M. GILLESPIE,* 1858, in absence of WM. CHAUVENET.*
13. WILLIAM CHAUVENET,* 1859.
14. JOSEPH LECONTE, 1860.
15. ELIAS LOOMIS,* 1866, in the absence of W. P. TROWBRIDGE*
16. C. S. LYMAN,* 1867.
17. SIMON NEWCOMB, 1868, in absence of A. P. ROCKWELL.
18. O. C. MARSH, 1869.
19. F. W. PUTNAM, 1870, in absence of C. F. HARTT.*
20. F. W. PUTNAM, 1871.
21. EDWARD S. MORSE, 1872.
22. C. A. WHITE, 1873.
23. A. C. HAMLIN, 1874.
24. S. H. SCUDDER, 1875.
25. T. C. MENDENHALL, 1876.
26. AUG. R. GROTE, 1877.
27. H. C. BOLTON, 1878.
28. H. C. BOLTON, 1879, in the absence of GEORGE LITTLE.
29. J. K. REES, 1880.
30. C. V. RILEY,* 1881.
31. WILLIAM SAUNDERS, 1882.
32. J. R. EASTMAN, 1883.
33. ALFRED SPRINGER, 1884.
34. C. S. MINOT, 1885.
35. S. G. WILLIAMS, 1886.
36. WILLIAM H. PETTEE, 1887.
37. JULIUS POHLMAN, 1888.
38. C. LEO MEES, 1889.
39. H. C. BOLTON, 1890.
40. H. W. WILEY, 1891.
41. A. W. BUTLER, 1892.
42. T. H. NORTON, 1893.
43. H. L. FAIRCHILD, 1894.
44. JAS. LEWIS HOWE, 1895.
45. CHARLES R. BARNES, 1896.
46. ASAPH HALL, JR., 1897.
47. J. MCMAHON, 1898, in place of D. S. KELLICOTT,* deceased.
48. F. BEDELL, 1899.

Permanent Secretaries, 1851–

5–7. SPENCER F. BAIRD,* 1851–4.
8–17. JOSEPH LOVERING,* 1854–68.
18. F. W. PUTNAM, 1869, in the absence of J. LOVERING.*
19–21. JOSEPH LOVERING,* 1870–73.
22–46. F. W. PUTNAM, 1873–98.
47–48. L. O. HOWARD, 1898–99.

Assistant General Secretaries, 1882–1887.

31. J. R. EASTMAN, 1882.
32. ALFRED SPRINGER, 1883.
33. C. S. MINOT, 1884, in absence of E. S. HOLDEN.
34. S. G. WILLIAMS, 1885, in the absence of C. C. ABBOTT.
35. W. H. PETTEE, 1886.
36. J. C. ARTHUR, 1887.

Secretaries of the Council, 1888–

37. C. LEO MEES, 1888.
38. H. C. BOLTON, 1889.
39. H. W. WILEY, 1890.
40. A. W. BUTLER, 1891.
41. T. H. NORTON, 1892.
42. H. LEROY FAIRCHILD, 1893.
43. JAS. LEWIS HOWE, 1894.
44. CHARLES R. BARNES, 1895.
45. ASAPH HALL, JR., 1896.
46. D. S. KELLICOTT,* 1897.
47. FREDERICK BEDELL, 1898.
48. CHARLES BASKERVILLE, 1899.

Secretaries of Section A.—Mathematics, Physics and Chemistry, 1875–81.

24. { S. P. LANGLEY, 1875. / T. C. MENDENHALL, 1875.
25. A. W. WRIGHT, 1876.
26. H. C. BOLTON, 1877.
27. F. E. NIPHER, 1878.
28. J. K. REES, 1879.
29. H. B. MASON, 1880.
30. E. T. TAPPAN, 1881, in the absence of JOHN TROWBRIDGE.

Secretaries of Section B.— Natural History, 1875–81.

24. EDWARD S. MORSE, 1875.
25. ALBERT H. TUTTLE, 1876.
26. WILLIAM H. DALL, 1877.
27. GEORGE LITTLE, 1878.
28. WILLIAM H. DALL, 1879, in the absence of A. C. WETHERBY.
29. CHARLES V. RILEY,* 1880.
30. WILLIAM SAUNDERS, 1881.

SECRETARIES OF SUBSECTIONS, 1875–81.

Subsection of Chemistry.

24. F. W. CLARKE, 1875.
25. H. C. BOLTON, 1876.
26. P. SCHWEITZER, 1877.
27. A. P. S. STUART, 1878.
28. W. R. NICHOLS,* 1879.
29. C. E. MUNROE, 1880.
30. ALFRED SPRINGER, 1881, in the absence of R. B. WARDER.

Subsection of Entomology.

30. B. P. MANN, 1881.

Subsection of Anthropology.

24. F. W. PUTNAM, 1875.
25. OTIS T. MASON, 1876.
26, 27. United with Section B.
28, 29, 30. J. G. HENDERSON, 1879–81.

Subsection of Microscopy.

25. E. W. MORLEY, 1876.
26. T. O. SOMMERS, JR., 1877.
27. G. J. ENGELMANN, 1878.
28, 29. A. B. HERVEY, 1879-1880.
30. W. H. SEAMAN, 1881, in the absence of S. P. SHARPLES.

SECRETARIES OF THE SECTIONS, 1882–

Section A.—Mathematics and Astronomy.

31. H. T. EDDY, 1882.
32. G. W. HOUGH, 1883, in the absence of W. W. JOHNSON.
33. G. W. HOUGH, 1884.
34. E. W. HYDE, 1885.
35. S. C. CHANDLER, 1886.
36. H. M. PAUL, 1887.
37. C. C. DOOLITTLE, 1888.
38. G. C. COMSTOCK, 1889.
39. W. W. BEMAN, 1890.
40. F. H. BIGELOW, 1891.
41. WINSLOW UPTON, 1892.
42. C. A. WALDO, 1893, in the absence of A. W. PHILLIPS.
43. J. C. KERSHNER, 1894, in place of W. W. BEMAN, resigned.
44. ASAPH HALL, JR., 1895, in place of E. H. MOORE, resigned.
45. EDWIN B. FROST, 1896.
46. JAMES MCMAHON, 1897.
47. WINSLOW UPTON, 1898, in place of ALEXANDER ZIWET, resigned.
48. JOHN F. HAYFORD, 1899.

Section B.—Physics.

31. C. S. HASTINGS, 1882.
32. F. E. NIPHER, 1883, in the absence of C. K. WEAD.
33. N. D. C. HODGES, 1884.
34. B. F. THOMAS, 1885, in place of A. A. MICHELSON, resigned.
35. H. S. CARHART, 1886.
36. C. LEO MEES, 1887.
37. ALEX. MACFARLANE, 1888.
38. E. L. NICHOLS, 1889.
39. E. M. AVERY, 1890.
40. ALEX. MACFARLANE, 1891.
41. BROWN AYRES, 1892.
42. W. LECONTE STEVENS, 1893.
43. B. W. SNOW, 1894.
44. E. MERRITT, 1895.
45. FRANK P. WHITMAN, 1896.
46. FREDERICK BEDELL, 1897.
47. W. S. FRANKLIN, 1898, in place of E. B. ROSA, resigned.
48. WILLIAM HALLOCK, 1899.

SECRETARIES OF THE SECTIONS, CONTINUED.

Section C.—Chemistry.

31. ALFRED SPRINGER, 1882.
32. J. W. LANGLEY, 1883.
    W. MCMURTRIE, 1883.
33. H. CARMICHAEL, 1884, in the absence of R. B. WARDER.
34. F. P. DUNNINGTON, 1885.
35. W. MCMURTRIE, 1886.
36. C. S. MABERY, 1887.
37. W. L. DUDLEY, 1888.
38. EDWARD HART, 1889.
39. W. A. NOYES, 1890.
40. T. H. NORTON, 1891.
41. JAS. LEWIS HOWE, 1892.
42. H. N. STOKES, 1893, in the absence of J. U. NEF.
43. MORRIS LOEB, 1894, in place of S. M. BABCOCK, resigned.
44. W. P. MASON, 1895.
    W. O. ATWATER, 1895.
45. FRANK P. VENABLE, 1896.
46. P. C. FREER, 1897.
47. C. BASKERVILLE, 1898.
48. H. A. WEBER, 1899.

Section D.—Mechanical Science and Engineering.

31. J. BURKITT WEBB, 1882, in the absence of C. R. DUDLEY.
32. J. BURKITT WEBB, 1883, *pro tempore*.
33. J. BURKITT WEBB, 1884.
34. C. J. H. WOODBURY, 1885.
35. WILLIAM KENT, 1886.
36. G. M. BOND, 1887.
37. ARTHUR BEARDSLEY, 1888.
38. W. B. WARNER, 1889.
39. THOMAS GRAY, 1890.
40. WILLIAM KENT, 1891.
41. O. H. LANDRETH, 1892.
42. D. S. JACOBUS, 1893.
43. JOHN H. KINEALY, 1894.
44. H. S. JACOBY, 1895.
45. JOHN GALBRAITH, 1896.
46. JOHN J. FLATHER, 1897.
47. W. S. ALDRICH, 1898.
48. J. M. PORTER, 1899.

Section E.—Geology and Geography.

31. H. S. WILLIAMS, 1882, in the absence of C. E. DUTTON.
32. A. A. JULIEN, 1883.
33. E. A. SMITH, 1884.
34. G. K. GILBERT, 1885, in the absence of H. C. LEWIS.*
35. E. W. CLAYPOLE, 1886.
36. W. M. DAVIS, 1887, in the absence of T. B. COMSTOCK.
37. JOHN C. BRANNER, 1888.
38. JOHN C. BRANNER, 1889.
39. SAMUEL CALVIN, 1890.
40. W J MCGEE, 1891.
41. R. D. SALISBURY, 1892.
42. W. H. HOBBS, 1893, in place of R. T. HILL, resigned.
43. JED. HOTCHKISS, 1894, in place of W. M. DAVIS, resigned.
44. J. PERRIN SMITH, 1895.
45. W. N. RICE, 1896, in place of A. C. GILL, resigned.
46. C. H. SMYTH, JR., 1897.
47. WARREN UPHAM, 1898.
48. ARTHUR HOLLICK, 1899.

Section F.—Biology, 1882–92.

31. WILLIAM OSLER, 1882, in the absence of C. S. MINOT.
32. S. A. FORBES, 1883.
33. C. E. BESSEY, 1884.
34. J. A. LINTNER, 1885, in place of C. H. FERNALD, resigned.
35. J. C. ARTHUR, 1886.
36. J. H. COMSTOCK, 1887.
37. B. H. PEEKNOW, 1888.
38. A. W. BUTLER, 1888.
39. J. M. COULTER, 1890.
40. A. J. COOK, 1891.
41. B. D. HALSTED, 1892.

Section F.—Zoölogy.

42. L. O. HOWARD, 1883.
43. JOHN B. SMITH, 1884, in place of WM. LIBBY, JR., resigned.
44. C. W. HARGITT, 1885, in place of S. A. FORBES, resigned.
45. D. S. KELLICOTT, 1886.
46. C. C. NUTTING, 1887.
47. R. T. JACKSON, 1888, in place of C. W. STILES, resigned.
48. F. W. TRUE, 1889.

SECRETARIES OF THE SECTIONS, CONTINUED.

Section G.—Microscopy, 1882–85.

31. ROBERT BROWN, JR. 1882.
32. CARL SEILER, 1883.
33. ROMYN HITCHCOCK, 1884.
34. W. H. WALMSLEY, 1885.

Section G.—Botany.

42. B. T. GALLOWAY, 1893, in the absence of F. V. COVILLE.
43. CHARLES R. BARNES, 1894.
44. B. T. GALLOWAY, 1895. M. B. WAITE, 1895.
45. GEORGE F. ATKINSON, 1896.
46. F. C. NEWCOMBE, 1897.
47. ERWIN F. SMITH, 1898.
48. W. A. KELLERMAN, 1899.

Section H.—Anthropology.

31. OTIS T. MASON, 1882.
32. G. H. PERKINS, 1883.
33. G. H. PERKINS, 1884, in the absence of W. H. HOLMES.
34. ERMINNIE A. SMITH,* 1885.
35. A. W. BUTLER, 1886.
36. CHARLES C. ABBOTT, 1887, in absence of F. W. LANGDON.
37. FRANK BAKER, 1888.
38. W. M. BEAUCHAMP, 1889.
39. JOSEPH JASTROW, 1890.
40. W. H. HOLMES, 1891.
41. W. M. BEAUCHAMP, 1892, in place of S. CULIN, resigned.
42. WARREN K. MOOREHEAD, 1893.
43. A. F. CHAMBERLIN, 1894.
44. STEWART CULIN and W. W. TOOKER, 1895, in place of ANITA N. MCGEE, resigned.
45. G. H. PERKINS, 1896, in place of J. G. BOURKE,* deceased.
46. ANITA N. MCGEE, 1897, in place of HARLAN I. SMITH, resigned.
47. MARSHALL H. SAVILLE, 1898.
48. GEORGE A. DORSEY, 1899.

Section I.—Social and Economic Science.

31. FRANKLIN B. HOUGH,* 1882. J. RICHARDS DODGE, 1882.
32. JOSEPH CUMMINGS,* 1883.
33. CHARLES W. SMILEY, 1884.
34. CHARLES W. SMILEY, 1885, in absence of J. W. CHICKERING.
35. H. E. ALVORD, 1886.
36. W. R. LAZENBY, 1887.
37. CHARLES S. HILL, 1888.
38. J. RICHARDS DODGE, 1889.
39. B. E. FERNOW, 1890.
40. B. E. FERNOW, 1891.
41. HENRY FARQUHAR, 1892, in place of L. F. WARD made Vice-president.
42. NELLIE S. KEDZIE, 1893.
43. MANLEY MILES, 1894.
44. W. R. LAZENBY, 1895, in place of E. A. ROSS, resigned.
45. R. T. COLBURN, 1896.
46. ARCHIBALD BLUE, 1897.
47. MARCUS BENJAMIN, 1898.
48. CALVIN M. WOODWARD, 1899.

TREASURERS.

1. JEFFRIES WYMAN,* 1848.
2. A. L. ELWYN,* 1849.
3. ST. J. RAVENEL,* 1850, in the absence of A. L. ELWYN.*
4. A. L. ELWYN,* 1850.
5. SPENCER F. BAIRD,* 1851, in absence of A. L. ELWYN.*

6–7. A. L. ELWYN,* 1851–1853.

8. J. L. LECONTE,* 1854, in absence of A. L. ELWYN.*

9–19. A. L. ELWYN,* 1855–1870.

20–30. WILLIAM S. VAUX,* 1871–1881.

32–42. WILLIAM LILLY,* 1882–1893.

43–47. R. S. WOODWARD, 1894–99.

COMMONWEALTH OF MASSACHUSETTS.

IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

AN ACT

To Incorporate the "American Association for the Advancement of Science."

Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:

Section 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be, possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

Section 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

Section 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

Section 4. This act shall take effect upon its passage.

House of Representatives, March 13, 1874.
Passed to be enacted,
John E. Sanford, Speaker.

In Senate, March 17, 1874.
Passed to be enacted. March 19, 1874.
Geo. B. Loring, President. Approved.
W. B. Washburn.

Secretary's Department,
Boston, April 8, 1874.
A true copy, Attest:
David Pulsifer,
Deputy Secretary of the Commonwealth.

CONSTITUTION

OF THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Incorporated by Act of the General Court of the Commonwealth of Massachusetts.

OBJECTS.

ARTICLE 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS, FELLOWS, PATRONS AND HONORARY FELLOWS.

ART. 2. The Association shall consist of Members, Fellows, Patrons, Corresponding Members and Honorary Fellows.

ART. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, and election by the Council. Any incorporated scientific society or institution, or any public or incorporated library, may be enrolled as a member of the Association by vote of the Council by payment of the initiation fee; such society, institution or library may be represented by either the President, Curator, Director or Librarian presenting proper credentials at any meeting of the Association for which the assessment has been paid.

ART. 4. Fellows shall be elected by the Council from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members of the Council at a designated meeting of the Council.

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, not exceeding three for each section, may be elected; the nominations to be made by the Council and approved by ballot in the respective sections before election by ballot in General Session. Honorary Fellows shall be entitled to all the privileges of Fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election. Corresponding Members shall consist of such scientists not residing in America as may be elected by the Council, and their number shall be limited to fifty. Corresponding Members shall be entitled to all the privileges of members and to the annual volumes of Proceedings published subsequent to their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected. The Council shall have power to exclude from the Association any member or fellow, on satisfactory evidence that said member or fellow is an improper person to be connected with the Association, or has in the estimation of the Council made improper use of his membership or fellowship.

ART. 8. No member or fellow shall take part in the organization of, or hold office in, more than one section at any one meeting.

OFFICERS.

ART. 9. The officers of the Association shall be elected by ballot by the Nominating Committee from the fellows, and shall consist of a President, a Vice President from each section, a Permanent Secretary, a General Secretary, a Secretary of the Council, a Treasurer, and a Secretary of each Section; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The term of office of Permanent Secretary shall be five years.

ART. 10. The President, or, in his absence, the senior Vice President present, shall preside at all General Sessions of the Association and at all meetings of the Council. It shall also be the duty of the President to give an address at a General Session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be chairmen of their respective

Sections, and of their Sectional Committees, and it shall be part of their duty to give an address, each before his own section, at such time as the council shall determine. The Vice Presidents may appoint temporary Chairmen to preside over the sessions of their sections, but shall not delegate their other duties. The Vice Presidents shall have seniority in order of their continuous membership in the Association.

ART. 12. The General Secretary shall be the Secretary of all General Sessions of the Association, and shall keep a record of the business of these sessions. He shall receive the records from the Secretaries of the Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting.

ART. 13. The Secretary of the Council shall keep the records of the Council. He shall give to the Secretary of each Section the titles of papers assigned to it by the Council. He shall receive proposals for membership and bring them before the Council.

ART. 14. The Permanent Secretary shall be the executive officer of the Association under the direction of the Council. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually a general report for publication in the annual volume of Proceedings. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least three months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Council, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Council, and shall pay over to the Treasurer such unexpended funds as the Council may direct. He shall receive and hold in trust for the

Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Council. He shall receive all communications addressed to the Association during the intervals between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Council, and may employ one or more clerks at such compensation as may be agreed upon by the Council.

ART. 15. The Treasurer shall invest the funds received by him in such securities as may be directed by the Council. He shall annually present to the Council an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Council, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Council. The Treasurer shall give bonds for the faithful performance of his duty in such manner and sum as the Council shall from time to time direct.

ART. 16. The Secretaries of the Sections shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the Secretaries of the Sectional Committees. The Secretaries shall have seniority in order of their continuous membership in the Association.

ART. 17. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Council as the President of the meeting. Vacancies in the offices of Vice President, Permanent Secretary, General Secretary, Secretary of the Council, and Treasurer, shall be filled by the Council by ballot. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the Section.

ART. 18. The Council shall consist of the past Presidents, and the Vice Presidents of the last meeting, together with the President, the Vice Presidents, the Permanent Secretary, the General Secretary, the Secretary of the Council, the Secretaries of the Sections, and the Treasurer of the current meeting, with the addition of one fellow elected from each Section by ballot on the first day of its meeting. The members present at any regularly called meeting of the Council, provided there are at least five,

shall form a quorum for the transaction of business. The Council shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Council shall be held in the Council room at 9 o'clock, A.M., on each day of the meeting of the Association. Special meetings of the Council may be called at any time by the President. The Council shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Council. The Council shall receive and assign papers to the respective sections; examine and, if necessary, exclude papers; decide which papers, discussions and other proceedings shall be published, and have the general direction of the publications of the Association; manage the financial affairs of the Association; arrange the business and programmes for General Sessions; suggest subjects for discussion, investigation or reports; elect members and fellows; and receive and act upon all invitations extended to the Association and report the same at a General Session of the Association. The Council shall receive all reports of Special Committees and decide upon them, and only such shall be read in General Session as the Council shall direct. The Council shall appoint at each meeting the following sub-committees who shall act, subject to appeal to the whole Council, until their successors are appointed at the following meeting: 1, on Papers and Reports; 2, on Members; 3, on Fellows.

ART. 19. The Nominating Committee shall consist of the Council, and one member or fellow elected by each of the Sections. It shall be the duty of this Committee to meet at the call of the President and elect the general officers for the following meeting of the Association. It shall also be the duty of this Committee to fix the time and place for the next meeting. The Vice President and Secretary of each Section shall be recommended to the Nominating Committee by a sub-committee consisting of the Vice President, Secretary, and three members or fellows elected by the Section.

MEETINGS.

ART. 20. The Association shall hold a public meeting annually, for one week or longer, at such time and place as may be determined by vote of the Nominating Committee, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Council may designate.

ART. 21. A General Session shall be held at 10 o'clock, A. M., on the first day of the meeting, and at such other times as the Council may direct.

SECTIONS AND SUBSECTIONS.

ART. 22. The Association shall be divided into Sections, namely:—A, *Mathematics and Astronomy;* B, *Physics;* C, *Chemistry, including its application to agriculture and the arts;* D, *Mechanical Science and Engineering;* E, *Geology and Geography;* F, *Zoölogy;* G, *Botany;* H, *Anthropology;* I, *Social and Economic Science.* The Council shall have power to consolidate any two or more Sections temporarily, and such consolidated Sections shall be presided over by the senior Vice President and Secretary of the Sections comprising it.

ART. 23. Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary and the Vice President and Secretary of the preceding meeting shall form its Sectional Committee. The Sectional Committees shall have power to fill vacancies in their own numbers. Meetings of the Sections shall not be held at the same time with a General Session.

ART. 24. The Sectional Committee of any Section may at its pleasure form one or more temporary Subsections, and may designate the officers thereof. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section.

ART. 25. A paper shall not be read in any Section or Subsection until it has been received from the Council and placed on the programme of the day by the Sectional Committee.

SECTIONAL COMMITTEES.

ART. 26. The Sectional Committees shall arrange and direct the business of their respective Sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Council. No change shall be made in the programme for the day in a Section without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself, must be returned to the Council with the reasons why it was refused.

ART. 27. The Sectional Committees shall examine all papers and abstracts referred to the Sections, and they shall not place on the programme

any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

Papers and Communications.

Art. 28. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Council to the Sectional Committee until an abstract of the paper or the paper itself has been received.

Art. 29. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

Art. 30. Whenever practicable, the proceedings and discussions at General Sessions, Sections and Subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the Secretaries.

Printed Proceedings.

Art. 31. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers or abstracts ready for the press, and these must be in the hands of the Secretaries of the Sections before the final adjournment of the meeting, otherwise only the titles will appear in the printed volume. The Council shall have power to order the printing of any paper by abstract or title only. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Council. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Council. The Council shall also designate the institutions to which copies shall be distributed.

Local Committee.

ART. 32. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

Library of the Association.

ART. 33. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets in circulation must be returned at each meeting. Not more than five books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Council. [The Library of the Association was, by vote of the Council in 1895, placed on deposit in the Library of the University of Cincinnati, Ohio. Members can obtain the use of books by writing to the Librarian of the University Library, Cincinnati, Ohio.]

Admission Fee and Assessments.

ART. 34. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

ART. 35. The annual assessment for members and fellows shall be three dollars.

ART. 36. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member, and as such, shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which, during the life of the member, shall form a part of the general fund of the Association; but, after his death, shall be used only to assist in original research, unless otherwise directed by unanimous vote of the Council.

ART. 37. All fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

ACCOUNTS.

ART. 38. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by Auditors appointed by the Council.

ALTERATIONS OF THE CONSTITUTION.

ART. 39. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in General Session, after notice given at a General Session of a preceding meeting of the Association.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRONS.[2]

THOMPSON, MRS. ELIZABETH, Stamford, Conn. (22).
LILLY, GEN. WILLIAM, Mauch Chunk, Pa. (28). (Died Dec. 1, 1893.)
HERRMAN, MRS. ESTHER, 59 West 56th St., New York, N. Y. (29).

CORRESPONDING MEMBERS.[3]

Warington, Robert, F.R.S., Rothamsted, Harpenden, England (40). C

MEMBERS.[4]

Abbe, Cleveland, jr., 2017 I St., N. W., Washington, D.C. (44). E
Abraham, Abraham, Brooklyn, N. Y. (43).
Abraham, Walter D., 1833 Venango St., Tioga, Philadelphia, Pa. (47).
Adams, Comfort A., Harvard University, Cambridge, Mass. (47).
Adams, C. E., M.D., 29 West Broadway, Bangor, Me. (43). F

[1] The numbers in parentheses indicate the meeting at which the member was elected. The black letters at the end of line indicate the sections to which members elect to belong. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings bound in paper. *The payment of ten dollars at one time entitles a member to the subsequent volumes to which he may be entitled, bound in cloth, or by the payment of twenty dollars, to such volumes bound in half morocco.*

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, and are entitled to the privileges of members and to the publications.

The names of Patrons are to remain permanently on the list.

[3] See ARTICLE VI of the Constitution.

[4] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The income of the money derived from a Life Membership is used for the general purposes of the Association during the life of the member; afterwards it is to be used to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.

Alden, John, Pacific Mills, Lawrence, Mass. (36).
Aldis, Owen F., 230 Monadnock Block, Chicago, Ill. (41). H
Allderdice, Wm. H., P. A. Engineer, U. S. Navy, care Navy Department, Washington, D. C. (33). B
Allen, Miss Augusta A., 42 Coulter St., Germantown, Pa. (44). G
Allen, J. M., Hartford, Conn. (22). B
Allen, John Robins, 18 S. Ingalls St., Ann Arbor, Mich. (45). B B
Allen, Walter S., 34 S. Sixth St., New Bedford, Mass. (39). C I
Anderson, Alexander P., Botanist, Clemson College, S. C. (45). G
Anderson, Frank P., Epworth, Iowa (46).
Andrews, Wm. C., care Claflin Thayer & Co., P. O. Box 2000, New York, N. Y. (46).
Anthony, Mrs. Emilia C., Gouverneur, St. Lawrence Co., N. Y. (47). G
Appleton, Rev. Edw. W., D.D., Ashbourne, Montgomery Co., Pa. (28).
Archambault, U. E., P. O. Box 1944, Montreal, P. Q., Can. (31).
Ashe, W. W., State Geological Survey, Raleigh, N. C. (47).
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26). B
Atwood, Dr. Charles, Moravia, N. Y. (45). G
AVERY, SAMUEL P., 4 E. 38th St., New York, N. Y. (36).
Ayer, Edward Everett, Room 12, The Rookery, Chicago, Ill. (37). H
Ayres, Horace B., Carlton, Minn. (40).

Baker, A. G., Springfield, Mass. (44).
Baker, O. M., 499 Main St., Springfield, Mass. (44).
Baker, Prof. Thos. R., Winter Park, Fla. (47).
Balch, Samuel W., Yonkers, N. Y. (43).
Balderston, C. Canby, 33 S. Tenth St., Philadelphia, Pa. (33). B
Baldwin, Mrs. G. H., 3 Madison Ave., Detroit, Mich. (34). H
Baldwin, Herbert B., 906 Broad St., Newark, N. J. (43).
BALDWIN, S. PRENTISS, 736 Prospect St., Cleveland, Ohio (47). E
Bancroft, Alonzo C., Elma, Erie Co., N. Y. (41).
BANGS, LEMUEL BOLTON, M.D., 127 E. 34th St., New York, N. Y. (36).
Bangs, Outram, 99 Pemberton Square, Boston, Mass. (47). F
Bannan, John F., North Andover, Mass. (44). C
Barbour, Prof. Ervin H., Univ. of Nebraska, Lincoln, Neb. (45). E
Bardwell, Elisabeth M., Mt. Holyoke College, South Hadley, Mass. (46).
BARGE, H. F., Manch Chunk, Pa. (33).
Barker, Mrs. Martha M., 26 Eleventh St., Lowell, Mass. (37). E H
Barker, Mrs. Mary E., Collinsville, Conn. (43).
Barnett, Miss Katie Porter, Madison, Georgia (44). A H
Barnhart, Arthur M., 185 Monroe St., Chicago, Ill. (42).
Barrows, Franklin W., 43 Park St., Rudson, N. Y. (47). F
Barton, G. E., 27 Main St., Millville, N. J. (46). C
Barton, George Hunt, 16 Lexington Ave., Cambridge, Mass. (47).
Barton, Prof. Samuel M., Kernstown, Va. (43). A
Bartow, Edward, Ph.D., Kansas State University, Lawrence, Kan. (47). C
Barwell, John William, Waukegan, Ill. (41).

Bashore, Dr. Harvey B., West Fairview, Pa. (46). **E**
Bates, George Williams, 32033 Buhl Block, Detroit, Mich. (46). **H I**
Bates, Miss Octavie Williams, A.B., LL.M., 53 Bagg St., Detroit, Mich. (47). **I**
Batterson, J. G., Hartford, Conn. (23).
Bausch, Henry, P. O. Drawer 1033, Rochester, N. Y. (41).
Baxter, James N., care H. E. and C. Baxter, cor. Division and Bedford Sts., Brooklyn, N. Y. (36).
Beach, Spencer Ambrose, N. Y. Experiment Station, Geneva, N. Y. (41). **G**
Bean, Thos. E., Box 441, Galena, Ill. (28). **F**
Becher, Franklin A., 406 Irving Place, Milwaukee, Wis. (41). **I A**
Beckwith, Miss Florence, 394 Alexander St., Rochester, N. Y. (45). **G**
Bell, Miss Clara, Springfield, Mass. (43).
Bell, C. M., M.D., 320 Fifth Ave., New York, N. Y. (36).
Benneson, Miss Cora Agnes, A.M., LL.B., 4 Mason St., Cambridge, Mass. (47). **I H**
Bennett, Rev. N. E., Wilmington, Ohio (47). **A**
Benneville, James S. de, 123 S. 7th St., Philadelphia, Pa. (46). **C**
Berry, John Wilson, C.E., Pittston, Pa. (47). **A B D E I**
Bevier, Miss Isabel, Plymouth, Ohio (46). **C**
Beyer, Samuel W., Iowa Agricultural College, Ames, Ia. (47) **E**
Bickford, Miss Elizabeth E., Ph.D., Vassar College, Poughkeepsie, N. Y. (46). **F**
Biddle, James G., 944 Drexel Building, Philadelphia, Pa. (33).
Bien, Julius, 140 Sixth Ave., New York, N. Y. (34). **E H**
Biggar, Hamilton F., M.D., 176 Euclid Ave., Cleveland, Ohio (40). **B F**
Bishop, Heber R., Mills Building, New York, N. Y. (36).
Bitting, Prof. A. W., Purdue Univ., Lafayette, Ind. (46.)
Blair, Mrs. Eliza N., Manchester, N. H. (40).
Blair, Mrs. Helen Quinche, The Ortiz, East Fourth and Sycamore Sts., Cincinnati, Ohio (40). **C**
Blish, W. G., Niles, Mich. (33). **B D**
Blount, Henry Fitch, "The Oaks," Washington, D. C. (32). **I B**
Bodine, Donaldson, Prof. of Zoölogy and Geology, Wabash College, Crawfordsville, Ind. (45). **E F**
Bogert, Marston Taylor, Havemeyer Hall, Columbia University, New York, N. Y. (47). **C**
Bogue, Rev. Horace P. V., Avon, N. Y. (41). **H I**
Bohannan, Prof. Rosser Daniel, Ohio State University, Columbus, Ohio (46). **A**
Boland, Miss Mary A., 117 West Newton St., Boston, Mass. (47).
Bonwill, W. G. A., D.D.S., 2009 Chestnut St., Philadelphia, Pa. (47). **F H**
Bookman, Samuel, Ph.D., 9 East 62d St., New York, N. Y. (47). **C**
Booraem, J. V. V., 204 Lincoln Place, Brooklyn, N. Y. (36).
Borner, William, The Majestic, 52 and 54 Walton Place, Chicago, Ill. (44).
Boutwell, John Mason, 10 Weld Hall, Cambridge, Mass. (46). **E**
Bowker, R. R., 28 Elm St., New York, N. Y. (43). **B**

Boyd, David, A.M., Greeley, Col. (46). I
Boyd, James E., Albion, Mich. (46). B
Boyd, Prof. James Harrington, University of Chicago, Chicago, Ill. (47). A
Boynton, May O., Ph.B., 69 North Prospect St., Burlington, Vt. (44). C
Brace, Miss E. M., 40 Cortlandt St., Rochester, N. Y. (47).
BRACKENRIDGE, GEO. W., San Antonio, Texas (41). I
Brackett, Byron B., Ph.D., 722 Union St., Schenectady, N. Y. (46). B
Brackett, Solomon H., St. Johnsbury, Vt. (47). B
BRADLEY, ARTHUR C., Newport, N. H. (43).
Bradley, Charles S., 44 Broad St., New York, N. Y. (40).
BRADLEY, M. J., 36 Hart St., Brooklyn, N. Y. (43).
BRADLEY, MILTON, Springfield, Mass. (44). B
Bramwell, Geo. W., 335 Broadway, New York, N. Y. (43). D
Brassill, Miss Sarah Ellen, So. Weymouth, Mass. (47). G F
Brayton, Miss Sarah H., M.D., Evanston, Ill. (33).
Brice, Judge Albert G., 19 Camp St., New Orleans, La. (32). H
Brigham, Prof. Albert P., Hamilton, Madison Co., N. Y. (41).
Britton, Wiley, Bureau of Pensions, Springfield, Mo. (40). F
Brown, Samuel B., Morgantown, W. Va. (40). E
Brownell, Silas B., 71 Wall St., New York, N. Y. (36).
Browning, Philip Embury, Kent Chemical Laboratory, Yale University, New Haven, Conn. (46). C
Bryant, Miss D. L., 998 Spring Garden St., Greensboro, N. C. (42). E
Buckingham, Chas. L., 195 Broadway, New York, N. Y. (28).
Buffum, Prof. Burt C., State University, Laramie, Wyo. (42). G
Burgess, Edward S., 11 W. 88th St., New York, N. Y. (47).
Burnham, Edward J., Manchester, N. H. (47). F
Burr, Mrs. Laura E., Commercial Hotel, Lansing, Mich. (34). B
Burton, Prof. Alfred E., Massachusetts Institute of Technology, Boston, Mass. (40). E
Burwell, Arthur W., Ph.D., 208 Superior St., Cleveland, Ohio (37).

Caldwell, Prof. Frank C., Ohio State University, Columbus, Ohio (46). B D
Calkins, Dr. Marshall, Springfield, Mass. (29).
Calvert, Prof. Sidney, M.A., University of Missouri, Columbia, Mo. (47). C
Cannon, George L., jr., High School, Denver, Col. (39). F H
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Smith, Arthur Whitmore, Middletown, Conn. (44). **B**
Smith, Benj. G., 45 Fayerweather St., Cambridge, Mass. (29). **I**
Smith, Miss Cora A., Lansingburg, N. Y. (44). **F G**
Smith, De Cost, Skaneateles, N. Y. (38). **H**
Smith, E. Reuel, Skaneateles, N. Y. (38).
Smith, Prof. Harold B., Worcester Polytechnic Institute, Worcester, Mass. (43). **D**
Smith, Prof. Herbert S. S., Princeton University, Princeton, N. J. (29). **D**
Smith, Herbert W., 2 Wabash Ave., Chicago, Ill. (45). **E**
Smith, Miss J. Angelina, State Normal School, Framingham, Mass. (45). **E**
Smith, James Hervey, Baldwin University, Berea, Ohio (40).
Smith, Miss Jane, Peabody Museum, Cambridge, Mass. (29). **H**
Smith, Jared G., Dept. of Agriculture, Washington, D. C. (47).
Smith, Jos. R., Col. and Ass't Surgeon Gen. U. S. A., 2300 De Laney Place, Philadelphia, Pa. (43).
Smith, Lee Herbert, M.D., 224 Elmwood Ave., Buffalo, N. Y. (45). **H**
Smith, Philip Sidney, 90 Elm Hill Ave , Roxbury, Mass. (47). **E**
Smith, Prof. Sarah Effie, Mt. Holyoke College, S. Hadley, Mass. (46). **A**
Smith, Prof. Thomas A., Beloit, Wis. (33). **B A**
Smith, Uselma C., 1515 Green St., Philadelphia, Pa. (33). **F**
Smith, William Lincoln, Mass. Institute Technology, Back Bay, Boston, Mass. (47).
Snyder, Miss Lillian, Lafayette, Ind. (47). **G**
Solotaroff, H., M.D., 251 East Broadway, New York, N. Y. (47) **H**
Soper, George A., 63 W. 96th St., New York, N. Y. (46). **C**
Soule, Wm., Ph.D., Alliance, Ohio (33). **B C E**
Souvielle, Mathieu, M.D., Box 355, Jacksonville, Fla. (36). **B E F**
Souvielle, Mrs. Mathieu, Box 355, Jacksonville, Fla. (24). **A B F**
Spalding, Fred. P., 159 E. Seneca St., Ithaca. N. Y. (46). **D**
Spofford, Paul N., P. O. Box 1667, New York, N. Y. (36).
Sprague, C. H., Malden, Mass. (29).
Squibb, Charles F., A.B., 150 Columbia Heights, Brooklyn, N. Y. (43).
Squibb, Edward Hamilton, M.D., 148 Columbia Heights, Brooklyn, N. Y.
Stahley, George D., A.M., M.D., Gettysburg, Pa. (47). **F G**
Stam, Colin F., Chestertown, Md. (33). **C F**
Stanton, Jonathan Y., Lewiston, Me. (47).

Stearns, John Brainerd, B.S., 44 S. Willard St., Burlington, Vt. (44). **C**
Stebbins, Miss Fannie A., 480 Union St., Springfield, Mass. (44). **G F**
Steiger, George, Chemical Laboratory, U. S. Geological Survey, Washington, D. C. (40). **C E B**
Stein, Dr. S. G., Muscatine, Iowa (43).
Stevens, Frank Lincoln, North High School, Columbus, Ohio (44). **G**
Stevens, Geo. T., M.D., 33 W. 33d St., New York, N. Y. (28). **B F**
Stewart, Fred. Carlton, Jamaica, N. Y. (44). **G**
Stewart, Oscar M., 23 Hazen St., Ithaca, N. Y. (46). **B**
Stickney, Gardner P., 427 Bradford St., Milwaukee, Wis. (44). **H**
Stillman, Prof. John M., Stanford University, Cal. (41).
Stockbridge, Horace E., Ph.D., Lake City, Fla. (47). **C I**
St. John, Charles E., Oberlin, Ohio (46). **B**
Stone, Lincoln R., M.D., Newton, Mass. (31).
Stoneman, Miss Bertha, Cornell University, Ithaca, N. Y. (45).
Stowell, John, 48 Main St., Charlestown, Mass. (21).
Stradling, Prof. George F., 3226 Arlington St., Philadelphia, Pa. (41).
Strong, Edwin A., State Normal School, Ypsilanti, Mich. (46). **B**
Strong, Wendell M., 307 Welch Hall, New Haven, Conn. (44). **A B**
Sullivan, J. A., 308 Main St., Malden, Mass. (27). **A**
Sweet, Henry N., 89 State St., Boston, Mass. (40). **H D**

Taft, Elihu B., Burlington, Vt. (36). **H**
Talbott, Mrs. Laura Osborne, 1445 Huntington Place, Washington, D. C. (36). **F G I**
Tanner, Prof. John Henry, 7 Central St., Ithaca, N. Y. (47). **A B**
Taylor, C. F., M.D., 1520 Chestnut St., Philadelphia, Pa. (45).
Taylor, Edward Randolph, Cleveland, Ohio (39). **C**
Taylor, Prof. Jas. M., Hamilton, Madison Co., N. Y. (33). **A D**
Taylor, Robert S., Box 2019, Fort Wayne, Ind. (39).
Ternan, James C., P. O. Drawer 1033, Rochester, N. Y. (43).
Thaw, Mrs. Mary Copley, Pittsburgh, Pa. (41). **H**
Thompson, Alton Howard, 721 Kansas Ave., Topeka, Kan. (33). **H**
Thompson, Mrs. Frank, 233 S. 4th St., Philadelphia, Pa. (33).
Thompson, Fred'k F., 283 Madison Ave., New York, N. Y. (36).
Thompson, J. L., M.D., Indianapolis, Ind. (39). **F**
Tight, Prof. William George, Granville, Ohio (39). **E**
Tilden, Dr. J. N., Peekskill, N. Y. (43).
Todd, Albert M., Kalamazoo, Mich. (37). **C**
Towle, Wm. Mason, State College, Center Co., Pa. (44). **D**
Townsend, Charles O., College Park, Md. (46).
Townsend, Franklin, 4 Elk St., Albany, N. Y. (4).
Treat, Erastus B., Publisher and Bookseller, 5 Cooper Union, cor. 4th Ave. and 8th St., New York, N. Y. (29). **F I**
Trowbridge, Rev. Chas. R., 223 Porter St., Easton, Pa. (46). **H I**
Trowbridge, Luther H., East Grand Circus Park, Detroit, Mich. (29).
Trowbridge, Mrs. M. E. D., East Grand Circus Park, Detroit, Mich. (21). **I G**

True, Rodney Howard, Wingra Park, Madison, Wis. (46). **G**
Trueblood, Mary Esther, Ph.M., Earlham College, Richmond, Ind. (47). **A**
Turner, J. Spencer, 109 Duane St., New York, N. Y. (43). **B**
Tyler, Dr. Harry Walter, 491 Boylston St., Boston, Mass. (47). **A**

Vail, Prof. Hugh D., Santa Barbara, Cal. (18).
Valentine, Edw. P., Richmond, Va. (33). **H**
Van Antwerp, Rev. Francis J., 26 Harper Ave., Detroit, Mich. (46).
VAN BEUREN, FREDERICK T., 21 W. 14th St., New York, N. Y. (36).
Van Brunt, Cornelius, 319 E. 57th St., New York, N. Y. (28).
Van Denberg, Joseph King, Fort Edward, N. Y. (47).
Van Slyke, Lucius L., Agric. Exper. Station, Geneva, N. Y. (41).
Varney, A. L., Major of Ordnance, U. S. A., Indianapolis Arsenal, Indianapolis, Ind. (44). **H**
VAUX, GEO., JR., 404 Girard Building, Philadelphia, Pa. (33). **E A**
Vermyné, J. J. B., M.D., 2 Orchard St., New Bedford, Mass. (29). **F**
Villard, Fanny G., Dobbs Ferry, N. Y. (36).
Vinal, W. Irving, 1106 East Capitol St., Washington, D. C. (40). **E**
Voorhees, Chas. H., M.D., 111 Carroll Place, New Brunswick, N. J. (29). **F H**
Vredenburgh, Edw. H., 122 S. Fitzhugh St., Rochester, N. Y. (29).

Wagner, George, University of Kansas, Lawrence, Kan. (46). **C F G**
Waldron, Clare B., Agricultural College, Fargo, N. D. (45).
Wales, Salem H., 25 E. 55th St., New York, N. Y. (36).
Walker, Byron Edmund, Toronto, Ontario, Can. (38). **E**
Walker, George C., Room 519, Rookery Building, Chicago, Ill. (17).
Walker, James, Seth Thomas Clock Co., 49 Maiden Lane, New York, N. Y. (43).
Wallace, Edwin Corby, Ph.G., foot of Sixth St., Long Island City, N. Y. (47).
Ward, Frank A., 16-26 College Ave., Rochester, N. Y. (40).
Ward, J. Langdon, 120 Broadway, New York, N. Y. (29). **I**
Ware, Miss Mary L., Brimmer St., Boston, Mass. (47).
Ware, Wm. R., Columbia University, New York, N. Y. (36).
Warner, Charles F., Manual Training School, Cambridge, Mass. (45). **B**
Warren, Howard C., Ph.D., Princeton, N. J. (46).
Warren, Mrs. Susan C., 67 Mt. Vernon St., Boston, Mass. (29).
Warrington, James N., 127 Park Ave., Chicago, Ill. (34). **D A B**
Washburn, Prof. F. L., State University, Eugene, Oregon (44). **F**
WATERS, GEO. F., 6 Somerset St., Boston, Mass. (29). **B F H E D**
Watson, Miss C. A., North Andover, Mass. (31). **D**
Watson, Thomas A., Weymouth, Mass. (42). **E**
Watters, William, M.D., 26 S. Common St., Lynn, Mass. (40). **E G**
Watts, A. J., M.D., 1123 Bedford Ave., Brooklyn, N. Y. (43).
Weaver, Gerrit E. Hambleton, A.M., 203 De Kalb Square, West Philadelphia, Pa. (38). **G I**

Webber, Herbert J., Dept. of Agriculture, Washington, D. C. (47).
Webster, Prof. Arthur Gordon, Clark University, Worcester, Mass. (47). **B A**
Webster, Mrs. N. B., Vineland, N. J. (48).
Weed, J. N., 71 Water St., Newburgh, N. Y. (37). **E I**
Weeden, Hon. Joseph E., Randolph, Cattaraugus Co., N. Y. (31).
Weeks, Fred Boughton, U. S. Geological Survey, Washington, D. C. (44). **E**
Weems, J. B., Ph.D., Iowa Agriculture College, Ames, Iowa (44). **C**
Weinzirl, John, 416 S. Arno St., Albuquerque, New Mex. (45). **G**
Welch, William Henry, M.D., 935 St. Paul St., Baltimore, Md. (47). **F H**
Wells, Frank, M.D., 178 Devonshire St., Boston, Mass. (47). **C**
Wells, Samuel, 31 Pemberton Square, Boston, Mass. (24). **H**
Wells, William H., jr., Lockport, N. Y. (39). **E**
Wernicke, Prof. Paul, 107 E. Maxwell St., Lexington, Ky. (44). **A B**
Werum, Jno. H., Toledo, Ohio (40). **E F**
Wetzler, Jos., 203 Broadway, New York, N. Y. (36).
Wheeler, T. B., M.D., 128 Metcalfe St., Montreal, P. Q., Can. (11).
Wheeler, William, C.E., Concord, Mass. (41).
Whetstone, John L., Summit Ave., Mt. Auburn, Cincinnati, Ohio (30). **D**
White, Charles G., Lake Linden, Mich. (46). **B C**
White, Charles H., Medical Director U. S. N., U. S. Naval Museum of Hygiene, Washington, D. C. (34). **C**
White, LeRoy S., Box 924, Waterbury, Conn. (23).
White, Thaddeus R., 257 W. 45th St., New York, N. Y. (42). **A**
White, Theodore Greely, Ph.B., A.M., Columbia University, New York, N. Y. (47) **B E**
Whitfield, Thomas, Ph.D., 240 Wabash Ave., Chicago, Ill. (41). **C**
Whiting, Mrs. Francis, 914 W. Lafayette St., Norristown, Pa. (40).
Whiting, S. B., 11 Ware St., Cambridge, Mass. (33). **D**
Whitney, Miss Mary W., Vassar College Observatory, Poughkeepsie N. Y. (47). **A**
Whitney, Willis Rodney, 29 Fairmount, Jamestown, N. Y. (46) **C**
Wiegand, Karl McKay, Ithaca, N. Y. (45). **G**
Wilbour, Mrs. Charlotte B., Little Compton, R. I. (28).
Wilcox, Miss Emily T., Meriden, Conn. (33). **B A**
Wilder, Geo. Walker, University of Wisconsin, Madison, Wis. (45). **B**
Willetts, Joseph C., Skaneateles, N. Y. (29). **E F H**
Williams, Charles B., Raleigh, N. C. (47).
Williams, Henry Smith, M.D., 165 W. 82nd St., New York, N. Y. (34). **F**
Williamson, Mrs. M. Burton, 1060 W. Jefferson St., Los Angeles, Cal. (44). **F**
WILMARTH, MRS. HENRY D., 51 Eliot St., Jamaica Plain, Mass. (40).
Wilmot, Thos. J., Commercial Cable Co., Waterville, County Kerry, Ireland (27). **B**
Wilson, Prof. Andrew G., Lenox College, Hopkinton, Iowa (48). **E**

Wilson, G. Reed, Townsend Block, Main cor. Swan St., Buffalo, N. Y. (45). **H**
Windsor, Sarah Sweet, A.B., M.D., 138 Marlboro St., Boston, Mass. (47). **F H**
Wingate, Miss Hannah S., 103 W. 132nd St., New York, N. Y. (31). **E I**
Witmer, Dr. Lightner, University of Pennsylvania, Philadelphia, Pa. (46). **H**
Wolcott, Mrs. Henrietta L. T., Dedham, Mass. (29).
Wolff, Frank A., jr., Ph.D., 1028 Vermont Ave. N. W., Washington, D. C. (47). **B**
Wood, Mrs. Cynthia A., 171 W. 47th St., New York, N. Y. (43).
Wood, Miss Elvira, 198 Adams St., Waltham, Mass. (47).
Wood, Rob't Williams, Physical Department, University of Wisconsin, 515 State St., Madison, Wis. (46).
WOOD, WALTER, 400 Chestnut St., Philadelphia, Pa. (33). **F I**
Woodbury, Charles T., North Andover, Mass (47) **C**
Woodhull, John Francis, Teachers' College, Morningside Heights, New York, N. Y. (43).
Woodrow, Miss Marion W., Winthrop Normal College, Rock Hill, S.C. (45). **E C**
Woodworth, William McMichael, Ph.D., 149 Brattle St., Cambridge, Mass. (44). **F**
Worcester, Dean C., Ann Arbor, Mich. (46). **F H**
Wright, Jonathan, M.D., 73 Remsen St., Brooklyn, N. Y. (43).
Wright, John S., care Eli Lilly & Co., Indianapolis, Ind. (42). **G**
Wright, Rufus, 338–339 Lake St., Chicago, Ill. (37). **B**
WUNDERLICH, FREDERICK W., M.D., 165 Remsen St., Brooklyn, N. Y. (45).

Yendell, Paul S., Winter St., Dorchester, Mass. (47). **A**
Youmans, Mrs. Celia G., Mount Vernon, N. Y. (36).
Youmans, Vincent J., Mount Vernon, N. Y. (43).

[962 PATRONS, CORRESPONDING MEMBERS AND MEMBERS.]

NOTE.—The omission of an address in the foregoing list indicates that letters mailed to that last printed were returned as uncalled for. Information of the present address of the members so indicated is requested by the PERMANENT SECRETARY.

SURVIVING FOUNDERS.

[At the Brooklyn Meeting, 1894, a resolution was unanimously adopted by which all surviving founders of the Association who have maintained an interest in science were made Honorary Life Members of the Association in recognition of their pioneer work in American Science.]

ABBOT, SAMUEL L., Boston, Mass.
BOYÉ, MARTIN H., Coopersburg, Pa.
DIXWELL, EPES S., Cambridge, Mass.
GIBBS, WOLCOTT, Newport, R. I.
HUBBARD, OLIVER PAYSON, New York, N. Y.
WEST, CHARLES E., Brooklyn, N. Y.

HONORARY FELLOWS.[1]

ROGERS, PROF. WILLIAM B., Boston, Mass. (1). 1881. (Born Dec. 7, 1804. Died May 30, 1882.) **B E**

CHEVREUL, MICHEL EUGÈNE, Paris, France (35). 1886. (Born Aug. 31, 1786. Died April 9, 1889.) **C**

GENTH, DR. F. A., 3937 Locust St., Philadelphia, Pa. (24). 1888. (Born May 17, 1820. Died Feb. 2, 1892.) **C E**

HALL, PROF. JAMES, Albany, N. Y. (1). 1890. (Born in 1811. Died Aug. 7, 1898.)

GOULD, DR. BENJAMIN APTHORP, Cambridge, Mass. (2). 1895. (Born Sept. 27, 1824. Died Nov. 26, 1896.) **A B**

LEUCKART, PROF. RUDOLF, (44). 1895. (Born in Helmstedt, Braunschweig, Germany, Oct. 7, 1823. Died in Leipzig, Feb. 7, 1898.) **F**

GIBBS, PROF. WOLCOTT, Newport, R. I. (1). 1896. **C B**

FELLOWS.[2]

Abbe, Professor Cleveland, Meteorologist, Weather Bureau, Department of Agriculture, Washington, D. C. (16). 1874. **B A**

Abbe, Dr. Robert, 11 W. 50th St., New York, N. Y. (36). 1892.

ABBOT, DR. SAMUEL L., 90 Mt. Vernon St., Boston, Mass. (1).

Abert, S. Thayer, Metropolitan Club, cor. 17 and H St., N. W., Washington, D. C. (30). 1891. **A B D E I**

Adriance, John S., 105 E. 39th St., New York, N. Y. (39). 1895. **C**

Aldrich, Prof. William Sleeper, West Virginia University, Department Mechanical Engineering, Morgantown, W. Va. (43). 1897. **D**

Alvord, Major Henry E., Lewinsville, Fairfax Co., Va. (29). 1882. **I**

Alwood, Prof. Wm. B., Agricultural and Mechanical College and Experiment Station, Blacksburg, Va. (39). 1891. **F**

Andrews, Prof. Launcelot W., Iowa City, Iowa (39). 1891. **C**

Anthony, Prof. Wm. A., Cooper Union, New York, N. Y. (28). 1880. **B**

Arthur, J. C., Lafayette, Ind. (21). 1883. **G**

Ashmead, Wm. H., 1833 M St., N. W., Washington, D. C. (40). 1892. **F**

Atkinson, Edward, 31 Milk St., Boston, Mass. (29). 1881. **I D**

Atkinson, George F., Cornell University, Ithaca, N. Y. (39). 1892. **G**

Atwater, Prof. W. O., Wesleyan University, Middletown, Conn. (29). 1882. **C**

Atwell, Charles B., 1938 Sherman Ave., Evanston, Ill. (36). 1890. **G**

Auchincloss, Wm. S., Bryn Mawr, Pa. (29). 1886. **D A**

Austen, Prof. Peter T., 99 Livingston St., Brooklyn, N. Y. (44). 1896. **C**

Avery, Elroy M., Ph.D., LL.D., 657 Woodland Hills Ave., Cleveland, Ohio (37). 1889. **B**

Ayres, Prof. Brown, Tulane University, New Orleans, La. (31). 1885. **B**

[1] See ARTICLE VI of the Constitution. [2] See ARTICLE IV of the Constitution.

*** The number in parenthesis indicates the meeting at which the member joined the Association; the date following is the year when made a Fellow; the black letters at end of line are those of the sections to which the Fellow belongs.

When the name is given in small capitals, it designates that the Fellow is also a Life Member.

Babcock, Prof. S. Moulton, Madison, Wis. (33). 1885. **C**

Bailey, E. H. S., Lawrence, Douglas Co., Kan. (25). 1889. **C E**

Bailey, Prof. Liberty H., Cornell University, Ithaca, N. Y. (34). 1887. **G**

Baker, Frank, M.D., 1804 Columbia Road, Washington, D. C. (31). 1886. **F H**

Baker, Marcus, U. S. Geological Survey, Washington, D. C. (30). 1882. **A**

Baldwin, Prof. J. Mark, Princeton University, Princeton, N. J. (46). 1898. **H**

Ballard, Harlan H., 50 South St., Pittsfield, Mass. (31). 1891. **E F**

Barker, Prof. G. F., 3909 Locust St., Philadelphia, Pa. (13). 1875. **B C**

Barnard, Edward E., care Yerkes Observatory, Lake Geneva, Williams Bay P.O., Wis. (26). 1883. **A**

Barnes, Chas. Reid, Ph.D., Prof. of Plant Physiology, University of Chicago, Chicago, Ill. (33). 1885. **G**

Barnum, Miss Charlotte C., Ph.D., 144 Humphrey St., New Haven, Conn. (36). 1896. **A**

Barrows, Walter B., Agricultural College, Ingham Co., Mich. (40). 1897. **F**

Bartlett, Prof. Edwin J., Dartmouth College, Hanover, N. H. (28). 1883. **C**

Bartlett, John R., Commander U. S. N., Lonsdale, R. I. (30). 1882. **E B**

Bartley, Elias H., M.D., 21 Lafayette Ave., Brooklyn, N. Y. (33). 1894. **C**

Barus, Carl, Ph.D., Wilson Hall, Brown University, Providence, R. I. (33). 1887. **B**

Bascom, Miss Florence, Bryn Mawr College, Bryn Mawr, Pa. (42). 1897. **E**

Baskerville, Charles, University of North Carolina, Chapel Hill, N. C. (41). 1894. **C E**

Bassett, Homer F., Waterbury, Conn. (23). 1874. **F**

Battle, Herbert B., Ph.D., Pres., Southern Chemical Co., Winston, N. C. (33). 1889. **C**

Bauer, Louis A., Ph.D., University of Cincinnati, Cincinnati, Ohio (40). 1892. **A**

Bausch, Edw., P. O. Drawer 1033, Rochester, N. Y. (26). 1883. **A B C F**

Beal, Prof. Wm. James, Agricultural College, Ingham Co., Mich. (17). 1880. **G**

Beardsley, Prof. Arthur, C.E., Ph.D., Swarthmore College, Swarthmore, Del. Co., Pa. (33). 1885. **D**

Beauchamp, Rev. Wm. M., Baldwinsville, N. Y. (34). 1886. **H**

Bedell, Frederick, Ph.D., Cornell University, Ithaca, N. Y. (41). 1894. **B A**

Bell, Alex. Melville, 1525 35th St., Washington, D. C. (31). 1885. **H**

Bell, Robert, M.D., Ass't Director Geological Survey, Ottawa, Ontario, Can. (38). 1889. **E F**

Beman, Wooster W., 19 S. 5th St., Ann Arbor, Mich. (34). 1886. **A**

Benjamin, Marcus, Smithsonian Institution, Washington, D. C. (27). 1887. **C**

Benjamin, Rev. Raphael, M.A., Hotel Premier, 72nd St., New York, N. Y. (34). 1887. **E F G H**

Bessey, Prof. Charles E., University of Nebraska, Lincoln, Neb. (21). 1880. **G**

Bethune, Rev. C. J. S., Trinity College School, Port Hope, Ontario, Can. (18). 1875. **F**

Bickmore, Prof. Albert S., American Museum of Natural History, 8th Ave. and 77th St., Central Park, New York, N. Y. (17). 1880. **H**

Bigelow, Prof. Frank H., U. S. Weather Bureau, Washington, D. C. (36). 1888. **A**

Bixby, Major W. H., U. S. A., Custom House, Cincinnati, Ohio (34). 1892. **D**

Blackham, George E., M.D., Dunkirk, N. Y. (25). 1883. **F**

Blair, Andrew A., 406 Locust St., Philadelphia, Pa. (44). 1896. **C**

Blake, Clarence J., M.D., 226 Marlborough St., Boston, Mass. (24). 1877. **B F**

Blake, Francis, Auburndale, Mass. (23). 1874. **B A**

Bleile, Albert M., M.D., Prof. of Physiology, State University, Columbus, Ohio (37). 1896. **F**

Blue, Archibald, Director of the Bureau of Mines, Toronto, Ontario, Can. (35). 1890. **I**

Boas, Dr. Franz, American Museum Natural History, Central Park, New York, N. Y. (36). 1888. **H I**

Boerner, Chas. G., Vevay, Switzerland Co., Ind. (29). 1886. **A B E**

Bolley, Henry L., North Dakota Experiment Station, Fargo, North Dakota (39). 1892. **G**

Bolton, Dr. H. Carrington, Cosmos Club, Washington, D. C. (17). 1875. **C**

Bond, Geo. M., care of The Pratt & Whitney Co., Hartford, Conn. (33). 1885. **D**

Booth, Miss Mary A., 32 Byers St., Springfield, Mass. 1894. (34). **F I G**

Bowditch, Charles P., 28 State St., Boston, Mass. (43). 1897. **H**

Bowditch, Prof. H. P., Jamaica Plain, Mass. (28). 1880. **F B H**

Bowser, Prof. E. A., Rutgers College, New Brunswick, N. J. (28). 1881.

Boyé, Martin H., M.D., Coopersburg, Lehigh Co., Pa. (1). 1896. **C**

Boynton, Prof. C. Smith, 69 N. Prospect St., Burlington, Vt. (44). 1896. **C**

Brackett, Prof. C. F., Princeton University, Princeton, N. J. (19). 1875. **B**

Brackett, Richard N., Associate Professor of Chemistry, Clemson College, S. C. (37). 1891. **C E**

Bradford, Royal B., Commander U. S. N., care Navy Department, Washington, D. C. (31). 1891. **B D**

Branner, Prof. John C., Stanford University, Cal. (34). 1886. **E F**

Brashear, Jno. A., Allegheny, Pa. (33). 1885. **A B D**

Brinton, D. G., M.D., Media, Pa. (33). 1885. **H**

Bristol, Wm. H., Stevens Institute, Hoboken, N. J. (36). 1894. **A B D**

Britton, N. L., Ph.D., Director-in-chief N. Y. Botanical Garden, 41 E. 49th St., New York, N. Y. (29). 1882. **G E**

Brooks, Wm. R., D.Sc., Director Smith Observatory, Geneva, N. Y. (35). 1886. **A B D G**

Brown, Robert, care of Yale University Observatory, New Haven, Conn. (11). 1874.

Brown, Mrs. Robert, New Haven, Conn. (17). 1874.

Brühl, Gustav, cor. John and Hopkins Sts., Cincinnati, Ohio (28). 1886. **H**

Brush, Charles F., Brush Electric Light Co., Cleveland, Ohio (35). 1886. **B**

Brush, Prof. George J., Yale Univ., New Haven, Conn. (4). 1874. **C E**

Buckhout, W. A., State College, Centre Co., Pa. (20). 1881. **F**

Bull, Prof. Storm, Madison, Wis. (44). 1897. **D**

Burgess, Dr. Thomas J. W., Med. Sup't, Protestant Hospital for the Insane, Montreal, P. Q., Can. (38). 1889. **G**

Burr, Prof. William H., School of Mines, 41 E. 49th St., New York, N. Y. (31). 1883.

Butler, A. W., Secretary Board of State Charities, Indianapolis, Ind. (30). 1885. **F H**

Caldwell, Prof. Geo. C., Cornell University, Ithaca, N. Y. (23). 1875. **C**

Calvin, Prof. Samuel, State University of Iowa, Iowa City, Iowa (37). 1889. **E F**

Campbell, Prof. Douglas H., Menlo Park, Cal. (34). 1888. **G**

Campbell, Prof. Edw. D., Ann Arbor, Mich. (44). 1896. **C**

Canby, William M., 1101 Delaware Avenue, Wilmington, Del. (17). 1878. **G**

Carhart, Prof. Henry S., University of Michigan, Ann Arbor, Mich. (29). 1881. **B**

Carpenter, Louis G., Agricultural College, Fort Collins, Col. (32). 1889. **A B**

Carter, James Madison G., M.D., Waukegan, Ill. (39). 1895. **F**

Carus, Paul, Ph.D., La Salle, Ill. (40). 1895. **H**

Casey, Thomas L., U. S. Engineer Office, Norfolk, Va. (38). 1892. **F**

Catlin, Charles A., 133 Hope St., Providence, R. I. (33). 1895. **C**

Cattell, Prof. James McKeen, Columbia University, New York, N. Y. (44). 1896. **B F H I**

Chalmot, G. de, Spray, N. C. (44). 1896. **C**

Chamberlin, T. C., 5041 Madison Ave., Chicago, Ill. (21). 1877. **E B F H**

Chandler, Prof. C. F., School of Mines, Columbia University, New York, N. Y. (19). 1875. **C**

Chandler, Prof. Charles Henry, Ripon, Wis. (28). 1883. **A**

Chandler, Seth C., 16 Craigie St., Cambridge, Mass. (29). 1882. **A**

Chanute, O., 413 E. Huron St., Chicago, Ill. (17). 1877. **D I**

Charbonnier, Prof. L. H., University of Georgia, Athens, Ga. (26). 1894. **A B D**

Chase, Frederick L., Yale University Observatory, New Haven, Conn. (43). 1896.

Cheney, Lellen Sterling, 1081 W. Johnson St., Madison, Wis. (42). 1894. **G**

Chester, Prof. Albert H., Rutgers College, New Brunswick, N. J. (29). 1882. **C F**

Chester, Commander Colby M , U. S. N., care Navy Department, Washington, D. C. (28). 1897. **E**

Chickering, Prof. J. W., Deaf Mute College, Washington, D. C. (22). 1877. **G I**

Christie, James, Pencoyd, Pa. (33). 1894. **D**

Chute, Horatio N., Ann Arbor, Mich. (34). 1889. **B C A**

Clapp, Miss Cornelia M., Mt. Holyoke College, South Hadley, Mass. (31). 1883. **F**

Clarke, Prof. F. W., U. S. Geological Survey, Washington, D. C. (18) 1874. **C**

Clark, Prof. John E., 445 Orange St., New Haven, Conn. (17). 1875. **A**

Clarke, John Mason, Ass't State Geologist and Palæontologist of N. Y., State Hall, Albany, N. Y. (45). 1897. **E**

Clarke, Robert, Glendale, Ohio (30). 1895. **H**

Clark, Wm. Bullock, Ph.D., Johns Hopkins University, Baltimore, Md. (37). 1891. **E**

Claypole, Prof. Edward W., Polytechnic Institute, Pasadena, Cal. (30). 1882. **E F**

Cloud, John W., 974 Rookery, Chicago, Ill. (28). 1886. **A B D**

Cochran, C. B., Food Inspector to State Board of Agriculture, 514 South High St., West Chester, Chester Co., Pa. (43). 1896. **C**

Coffin, Selden J., Prof. of Astronomy, Lafayette College, Easton, Pa. (22). 1874. **A I**

Cogswell, W. B., Syracuse, N. Y. (33). 1891. **D**

COLBURN, RICHARD T., Elizabeth, N. J. (31). 1894. **I F H**

Cole, Prof. Alfred D., Denison Univ., Granville, Ohio (39). 1891. **B C**

Collin, Prof. Alonzo, Cornell College, Mount Vernon, Iowa (21). 1891. **B C**

Collingwood, Francis, Elizabeth, N. J. (36). 1888. **D**

Colvin, Verplanck, Supt. N. Y. State Adirondack Survey, Albany, N. Y. (28). 1880. **E**

Conant, Prof. L. L., Polytechnic Institute, Worcester, Mass. (39). 1892. **A**

Cook, Prof. Orator F., U. S. National Museum, Washington, D. C. (40). 1892. **G**

Cooley, Prof. Le Roy C., Vassar College, Poughkeepsie, N. Y. (19). 1880. **B C**

Cooley, Prof. Mortimer E., University of Michigan, Ann Arbor, Mich. (33). 1885. **D**

Corthell, Elmer L., 27 Pine St., New York, N. Y. (34). 1886. **D I E**

Coulter, Prof. John M., University of Chicago, Chicago, Ill. (32). 1884. **G**

Coville, Frederick V., Department of Agriculture, Washington, D. C. (35). 1890. **G**

Cowles, Alfred H., 656 Prospect St., Cleveland, Ohio (37). 1897. **B C**

Crafts, James Mason, Pres. Mass. Institute Technology, Back Bay, Boston, Mass. (47). 1898. **C**

Cragin, Francis W., Colorado College, Colorado Springs, Col. (29). 1890. **F E H**

Crampton, Chas. A., M.D., Office of Internal Revenue, Treasury Department, Washington, D. C. (36). 1887. **C**

Crandall, Prof. Charles S., Fort Collins, Col. (40). 1894.

Crawford, Prof. Morris B., Middletown, Conn. (30). 1889. **B**

Crockett, Charles W., Rensselaer Polytechnic Institute, Troy, N. Y. (39). 1894. **A D**

Cross, Prof. Charles R., Massachusetts Institute Technology, Back Bay, Boston, Mass. (29). 1880. **B**

Cushing, Frank H., Bureau of Ethnology, Washington, D. C. (40). 1893. **H**

Cushing, Henry Platt, Adelbert College, Cleveland, Ohio (33). 1888. **E**

Dall, William H., Smithsonian Institution, Washington, D. C. (18). 1874. **H F**

Daly, Hon. Charles P., 84 Clinton Place, New York, N. Y. (36). 1898. **E H**

Dana, Edward Salisbury, New Haven, Conn. (23). 1875. **B E**

Dana, Gen. James J., U. S. A., 1412 21st St., N. W., Washington, D. C. (40). 1896.

Darton, Nelson H., U. S. Geological Survey, Washington, D. C. (37). 1893.

Davidson, Prof. George, Consulting Engineer, 530 California St., San Francisco, Cal. (29). 1881. **A B D**

Davis, Bradley Moore, Dep't of Botany, University of Chicago, Chicago, Ill. (45). 1897. **G**

Davis, C. H., Commander U. S. N., Navy Department, Washington, D. C. (40). 1896.

Davis, Prof. Wm. Morris, Cambridge, Mass. (33). 1885. **E B**

Dawson, Geo. M., S.S.C., F.G.S., Geol. Survey, Ottawa, Ontario, Can. (38). 1895. **E**

Dawson, Sir William, Principal McGill College, Montreal, Can. (10). 1875. **E**

Day, David F., Buffalo, N. Y. (35). 1887. **G**

Day, Fisk H., M.D., 309 Sycamore St., Lansing, Mich. (20). 1874. **E H F**

Dennis, Louis Monroe, Cornell University, Ithaca, N. Y. (43). 1895. **C**

Denton, Prof. James E., Stevens Institute, Hoboken, N. J. (36). 1888. **D B A**

Derby, Orville A., San Paulo, Brazil, S. A. (39). 1890.

Dewey, Frederic P., 702 9th St. N. W., Washington, D. C. (30). 1886. **C E**

Dexter, Julius, Cincinnati, Ohio (30).

Dixwell, Epes S., Cambridge, Mass. (1). 1896. **H F**

Dodge, Charles R., U. S. Dep't Agriculture, Washington, D. C. (22). 1874.

Dodge, Chas. W. M.S., Univ. of Rochester, Rochester, N. Y. (39). 1898. **F**

Dolbear, Prof. A. Emerson, Tufts College, Mass. (20). 1880. **B**

Doolittle, Prof. C. L., University of Pennsylvania, Philadelphia, Pa. (25). 1885. **A**

Dorsey, George A., Ph.D., Ass't in Anthropology Field Columbian Museum, Chicago, Ill. (39). 1892. **H**

Dorsey, N. Ernest, Ph.D., Annapolis Junction, Md. (46). 1898. **B**
Douglass, Andrew E., American Museum of Natural History, Central Park, New York, N. Y. (31). 1885. **H**
Draper, Dan'l, Ph.D., Director N. Y. Meteorological Observatory, Central Park, 64th St., Fifth Ave., New York, N. Y. (29). 1881. **B D F A**
Drown, Prof. Thos. M., Lehigh University, South Bethlehem, Pa. (29). 1881. **C**
Du Bois, Prof. Aug. J., New Haven, Conn. (30). 1882. **A B D**
Du Bois, Patterson, Ass't Editor S.S.T., 1081 Walnut St., Philadelphia, Pa. (33). 1887. **H C I**
Dudley, Charles B., Drawer 334, Altoona, Pa. (23). 1882. **C B D**
Dudley, Wm. L., Prof. of Chemistry, Vanderbilt University, Nashville, Tenn. (28). 1881. **C**
Dudley, Prof. Wm. R., Stanford University, Cal. (29). 1883. **G**
Dumble, E. T., 1708 Prairie Ave., Houston, Tex. (37). 1891. **E**
Dunham, Edw. K., 338 E. 26th St., New York, N. Y. (30). 1890.
Dunnington, Prof. F. P., University Station, Charlottesville, Va. (26). 1880. **C**
Du Pont, Francis G., Wilmington, Del. (33). 1896. **A B D**
Durand, Prof. W. F., Ph.D., Cornell University, Ithaca, N.Y. (37). 1890. **B**
Dwight, Prof. William B., Vassar College, Poughkeepsie, N. Y. (30). 1882. **E F**
Dyar, Harrison G., A.M., Ph.D., U. S. National Museum, Washington, D.C. (43). 1898.

Eastman, Charles Rochester, Museum Comparative Zoölogy, Cambridge, Mass. (41). 1896. **E F**
Eastman, Prof. J. R., Andover, N. H. (26). 1879. **A**
Eccles, Robert G., M.D., 191 Dean St., Brooklyn, N. Y. (31). 1894. **F C**
Eddy, Prof. H. T., Engineering and Mechanics, University of Minnesota, Minneapolis, Minn. (24). 1875. **A B D**
Edison, Thos. A., Orange, N. J. (27). 1878. **B**
Egleston, Prof. Thomas, 35 W. Washington Square, New York, N. Y. (27). 1879. **C D E**
Eichelberger, William Snyder, Ph.D., Nautical Almanac Office, U. S. Naval Observatory, Washington, D. C. (41). 1896. **A**
Eimbeck, William, U. S. Coast and Geodetic Survey, Washington, D. C. (17). 1874. **A B D**
Elkin, William L., Yale University Observatory, New Haven, Conn. (33). 1885. **A**
Ely, Theo. N., Chief of Motive Power, Pennsylvania R. R., Broad St. Station, Philadelphia, Pa. (29). 1886.
Emerson, Prof. Benjamin K., Box 203, Amherst, Mass. (19). 1877. **E F**
Emerson, Prof. C. F., Box 499, Hanover, N. H. (22). 1874. **B A**
Emery, Albert H., Stamford, Conn. (29). 1884. **D B**
Emery, Charles E., Bennett Building, New York, N. Y. (34). 1886. **D B A**

Emmons, S. F., U. S. Geological Survey, Washington, D. C. (26). 1879. **E**
Engelmann, George J., M.D., 336 Beacon St., Boston, Mass. (25). 1875. **F H**
Ewell, Ervin E., 3644 13th St., N. W., Washington, D. C. (40). 1896. **C**
Eyerman, John, "Oakhurst," Easton, Pa. (33). 1889. **E C**

Fairbanks, Henry, Ph.D., St. Johnsbury, Vt. (14). 1874. **B D A**
Fairchild, David Grandison, Dept. of Agriculture, Washington, D. C. (47). 1898. **G**
Fairchild, Prof. H. L., University of Rochester, Rochester, N. Y. (28). 1883. **E F**
Fanning, John T., Consulting Engineer, Kasota Block, Minneapolis, Minn. (29). 1885. **D**
Farlow, Dr. W. G., 24 Quincy St., Cambridge, Mass. (20). 1875. **G**
Farquhar, Henry, Dep't of Agric., Washington, D. C. (33). 1886. **A I G B**
Fernow, Bernhard E., Director of School of Forestry, Cornell University, Ithaca, N. Y. (31). 1887. **G I**
Ferry, Ervin S., University of Wisconsin, Madison, Wis. (41). 1896.
Firmstone, F., Easton, Pa. (33). 1887. **D**
Flather, Prof. John J., 160 South St., Lafayette, Ind. (44). 1896. **D**
Fletcher, Miss Alice C., care Peabody Museum, Cambridge, Mass. (29). 1883. **H**
Fletcher, James, Dominion Entomologist, Experimental Farm, Ottawa, Ontario, Can. (31). 1883. **F**
Fletcher, Dr. Robert, Army Medical Museum, Washington, D. C. (29). 1881. **F H**
Flint, Albert S., Washburn Observatory, Madison, Wis. (30). 1887. **A**
Flint, James M., Surgeon U. S. N., Smithsonian Institution, Washington, D. C. (28). 1882. **F**
Ford, Prof. D. R., Elmira, N. Y. (41). 1894. **A B**
Fox, Oscar C., U. S. Patent Office, Washington, D. C. (36). 1891. **B D A**
Franklin, William S., Lehigh University, So. Bethlehem, Pa. (36). 1892. **B**
Frazer, Dr. Persifor, Drexel Building, Room 1042, Philadelphia, Pa. (24). 1879. **E C**
Frazier, Prof. B. W., The Lehigh University, So. Bethlehem, Pa. (24). 1882. **E C**
Frear, Wm., State College, Centre Co., Pa. (33). 1886. **C**
Freer, Prof. Paul C., Ann Arbor, Mich. (39). 1891. **C**
French, Prof. Thomas, jr., Ridgeway Ave., Avondale, Cincinnati, Ohio (30). 1883. **B**
Frisby, Prof. Edgar, U. S. N. Observatory, Washington, D. C. (28). 1880. **A**
Frost, Edwin Brant, Yerkes Observatory, Williams Bay, Wis. (38). 1890. **A B**
Fuller, Prof. Homer T., President Drury College, Springfield, Mo. (35). 1891. **C E**

Fulton, Robert B., Chancellor University of Mississippi, Prof. of Physics and Astronomy, University, Miss. (21). 1887. **B A**

Gaffield, Thomas, 54 Allen St., Boston, Mass. (29). 1889. **C B**
Gage, Prof. Simon Henry, Ithaca, N. Y. (28). 1881. **F**
Galbraith, Prof. John, Toronto, Ontario, Can. (38). 1889. **D**
Gibbs, Prof. J. Willard, New Haven, Conn. (33). 1885. **B**
Gilbert, G. K., U. S. Geological Survey, Washington, D. C. (18). 1874. **E**
Gill, Adam Capen, Cornell University, Ithaca, N. Y. (38). 1894. **E**
Gill, Augustus Herman, Massachusetts Institute Technology, Back Bay, Boston, Mass. (44). 1896. **C**
Gill, Prof. Theo., Columbian University, Washington, D. C. (17). 1874. **F**
Gillman, Henry, 107 Fort St. West, Detroit, Mich. (24). 1875. **H F G**
Gilman, Daniel C., President Johns Hopkins University, Baltimore, Md. (10). 1875. **E H**
Glenn, William, 1348 Block St., Baltimore, Md. (33). 1893. **C**
Goessman, Prof. C. A., Massachusetts Agricultural College, Amherst, Mass. (18). 1875. **C**
Goff, E. S., 1113 University Ave., Madison, Wis. (35). 1889.
Gold, Theodore S., West Cornwall, Conn. (4). 1887. **B C**
Golden, Miss Katherine E., Lafayette, Ind. (42). 1897. **G**
Goldschmidt, S. A., Ph.D., 43 Sedgwick St., Brooklyn, N. Y. (24). 1880. **C E B**
Goldsmith, Edw., 658 N. 10th St., Philadelphia, Pa. (29). 1892. **C B**
Gooch, Frank A., Yale University, New Haven, Conn. (25). 1880. **C**
Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18). 1875. **G**
Goodspeed, Arthur Willis, A.B., Ph.D., University of Pennsylvania, Philadelphia, Pa. (47). 1898. **A B**
Goss, Prof. Wm. F. M., Lafayette, Ind. (39). 1896.
GRANT, MRS. MARY J., 36 Division St., Danbury, Conn. (23). 1874. **A**
Gratacap, L. P., Ph.B., 77th St. and 8th Ave., New York, N. Y. (27). 1884. **C E F**
Gray, Prof. Thomas, Terre Haute, Ind. (38). 1889. **D**
Green, Arthur L., Purdue University, Lafayette, Ind. (33). 1888. **C**
Grimes, J. Stanley, 1421 Wesley Ave., Evanston, Ill. (17). 1874. **E H**
Grindley, Harry Sands, 918 W. Green St., Urbana, Ill. (46). 1898. **C**
Grinnell, George Bird, 346 Broadway, New York, N. Y. (25). 1885. **F E**
Griswold, Leon Stacy, 238 Boston St., Dorchester, Mass. (38). 1893. **E**
Gruener, Hippolyte, Adelbert College, Cleveland, Ohio (44). 1898.
Gunckel, Lewis W., 435 W. Second St., Dayton, Ohio (41). 1897. **H**
Guthe, Karl E., Ph.D., Instructor in Physics at the University of Michigan, 904 S. State St., Ann Arbor, Mich. (45). 1897. **B D**

Hague, Arnold, U. S. Geological Survey, Washington, D. C. (26). 1879. **E**
Haines, Reuben, Haines and Chew Sts., Germantown, Philadelphia, Pa. (27). 1889. **C B**

Hale, Albert C., Ph.D., No. 551 Putnam Ave., Brooklyn, N. Y. (29). 1886. **C B**

Hale, Geo. E., Yerkes Observatory, Williams Bay, Wis. (37). 1891. **A B C**

Hale, William H., Ph.D., 40 First Place, Brooklyn, N. Y. (19). 1874. **I F H C B E A**

Hall, Arthur G., 1036 Oakland Ave., Ann Arbor, Mich. (41). 1896. **A B**

Hall, Prof. Asaph, 12 Kirkland Place, Cambridge, Mass. (25). 1877. **A**

Hall, Asaph, jr., University of Michigan, Ann Arbor, Mich. (38). 1890. **A**

Hall, Prof. C. W., Dean College Engineering Met. and Mechan. Arts, University of Minnesota, Minneapolis, Minn. (28). 1883. **D E**

Hall, Prof. Edwin H., 5 Avon St., Cambridge, Mass. (29). 1881. **B**

Hall, Prof. Lyman B., Haverford College, Haverford, Pa. (31). 1884. **C**

Hallock, Albert P., Ph.D., 440 First Ave., New York, N. Y. (31). 1896. **C**

Hallock, Dr. William, Columbia University, New York, N. Y. (40). 1893. **B E**

Hallowell, Miss Susan M., Wellesley College, Wellesley, Mass. (33). 1890. **G**

Halsted, Byron D., New Jersey Agricultural Experiment Station, New Brunswick, N. J. (29). 1883. **G**

Halsted, Prof. George Bruce, Austin, Texas (43). 1896.

Hambach, Dr. G., 1319 Lami St., St. Louis, Mo. (26). 1891. **F E**

Hamlin, Dr. A. C., Bangor, Me. (10). 1874. **C E H**

HANAMAN, C. E., Troy, N. Y. (19). 1883. **F**

Hardy, Prof. A. S., Dartmouth College, Hanover, N. H. (28). 1883. **A**

Hargitt, Prof. Charles W., Syracuse University, Syracuse, N. Y. (38). 1891. **F**

HARKNESS, PROF. WILLIAM, U. S. N. Observatory, Washington, D. C. (26). 1878. **A B C D**

Harris, Abram Winegardner, Sc.D., President Maine State College, Orono, Me. (40). 1895. **C**

Harris, Prof. E. P., Amherst College, Amherst, Mass. (44). 1896.

Harris, Gilbert D., Cornell University, Ithaca, N. Y. (37). 1893. **F E**

Harris, Uriah R., Lieutenant Com'dr, U. S. N., Navy Yard, Boston, Mass. (34). 1886. **A**

Hart, Edw., Ph.D., Lafayette College, Easton, Pa. (33). 1885. **C**

Haskell, Eugene E., U. S. Engineer Office, Telephone Building, Detroit, Mich. (39). 1896. **A B D**

HASTINGS, C. S., Sheffield Scientific School of Yale University, New Haven, Conn. (25). 1878. **B**

Haynes, Prof. Henry W., 239 Beacon St., Boston, Mass. (28). 1884. **H**

Hedrick, Henry B., A.B., Glencarlyn, Alexandria Co., Va. (40). 1896.

Hering, Rudolph, Civil and Sanitary Engineer, 100 William St., New York, N. Y. (33). 1885. **D E I**

Herty, Chas. Holmes, Ph.D., University of Georgia, Athens, Ga. (42). 1895. **C**

Hervey, Rev. A. B., Bath, Me. (22). 1879. **F**

Hilgard, Prof. E. W., University of California, Berkeley, Cal. (11). 1874. **C E B**

Hill, John Edward, Brown University, Providence, R. I. (44). 1897. **D**

Hill, Robert Thomas, U. S. Geological Survey, Washington, D. C. (36). 1889. **E**

Himes, Prof. Charles F., Carlisle, Pa. (29). 1882. **B C**

Hinrichs, Dr. Gustavus, 3132 Lafayette Ave., St. Louis, Mo. (17). 1874. **C B**

Hitchcock, Albert Spear, Manhattan, Kan. (39). 1892. **G**

HITCHCOCK, PROF. CHARLES H., Hanover, N. H. (11). 1874. **E**

Hitchcock, Romyn, 36 Wall St., New York, N. Y. (47). 1898. **C B**

Hobbs, William Herbert, Ph.D., Madison, Wis. (41). 1893. **E**

Hodgkins, Prof. H. L., Columbian University, Washington, D. C. (40). 1896. **A B**

Holland, W. J., D.D., LL.D., Chancellor Western University of Pennsylvania, Pittsburgh, Pa. (37). 1896. **F**

Hollick, Arthur, Columbia University, New York, N. Y. (31). 1892. **G E**

Holmes, Prof. Jos. A., Chapel Hill, N. C. (33). 1887. **E F**

Holmes, Wm. H., National Museum, Washington, D.C. (30). 1883. **H**

Hopkins, Thos. C., Geologist, State College, Centre Co., Pa. (38). 1898. **E**

Horsford, Miss Cornelia, 27 Craigie St., Cambridge, Mass. (46). 1897. **H**

Hough, Prof. G. W., Northwestern University, Evanston, Ill. (15). 1874. **A**

Hough, Walter, U. S. National Museum, Washington, D. C. (38). 1890.

Hovey, Edmund O., American Museum Natural History, Central Park, New York, N. Y. (36). 1895. **C E**

Hovey, Rev. Horace C., 60 High St., Newburyport, Mass. (29). 1883. **E H**

Howard, Prof. Curtis C., 97 Jefferson Ave., Columbus, Ohio (38). 1892. **C**

Howard, Leland O., Dep't of Agriculture, Washington, D. C. (37). 1889. **F**

Howe, Charles S., Prof. of Mathematics, Case School of Applied Science, Cleveland, Ohio (34). 1891. **A**

Howe, Prof. Jas. Lewis, Washington and Lee University, Lexington, Va. (36). 1888. **C**

Howell, Edwin E., 612 17th St., N. W., Washington, D. C. (25). 1891. **E**

Hrdlicka, Alēs, M.D., 130 E. 93d St., New York, N. Y. (46). 1897. **H**

Hubbard, Henry Guernsey, 230 New Jersey Ave., Washington, D. C. (41). 1895. **F**

HUBBARD, PROF. OLIVER PAYSON, 117 W. 55th St., New York, N. Y. (1) 1896.

Hunt, Alfred E., 116 Water St., Pittsburgh, Pa. (35). 1891. **C D**

Hunter, Andrew Frederick, Barrie, Ontario, Can. (38). 1896. **B H I**

Hyatt, Prof. Alpheus, Natural History Society, Back Bay, Boston, Mass. (18). 1875. **E F**

Hyde, Prof. E. W., Station D, Cincinnati, Ohio (25). 1881. **A**

Hyde, John, Statistician U. S. Dep't Agric., Washington, D. C. (47). 1898. **I E**

Iddings, Joseph P., University of Chicago, Chicago, Ill. (31). 1884. **E**

ILES, GEORGE, 5 Brunswick St., Montreal, Can. (31). 1898. **I**

Ives, Frederick E., 2750 N. 11th St., Philadelphia, Pa. (44). 1898. **B**

Jack, John G., Jamaica Plain, Mass. (31). 1890. **G**
Jackson, Prof. Charles L., Harvard University, Cambridge, Mass. (44). 1895. **C**
Jackson, Dr. Robert T., 33 Gloucester St., Boston, Mass. (37). 1890. **F**
Jacobus, David S., Stevens Institute, Hoboken, N. J. (36). 1889. **D B A**
Jacoby, Harold, Columbia University, New York, N. Y. (38). 1891. **A**
Jacoby, Henry S., Associate Prof. of Bridge Engineering and Graphics, Cornell University, Ithaca, N. Y. (36). 1892. **D**
Jastrow, Dr. Jos., University of Wisconsin, Madison, Wis. (35). 1887. **H F**
Jayne, Horace F., 1826 Chestnut St., Philadelphia, Pa. (29). 1884. **F H**
Jeffries, B. Joy, M.D., 15 Chestnut St., Boston, Mass. (29). 1881. **F H**
Jenkins, Edw. H., Drawer 101, New Haven, Conn. (33). 1885. **C**
Jenks, Elisha T., Middleborough, Mass. (22). 1874. **D**
Jesup, Prof. Henry G., Dartmouth College, Hanover, N. H. (36). 1891. **F**
Jesup, Morris K., 44 Pine St., New York, N. Y. (29). 1891. **I**
Jewell, Theo. F., Commander U. S. Navy, Room No. 22, P. O. Building, Buffalo, N. Y. (25). 1882. **B**
Jillson, Dr. B. C., 6045 Bond St., Pittsburgh, Pa. (14). 1881. **E H F**
Johnson, John B., Washington University, St. Louis, Mo. (33). 1886. **D**
Johnson, Otis C., 52 Thayer St., Ann Arbor, Mich. (34). 1886. **C**
Jones, Lewis R., Burlington, Vt. (41). 1894. **G**
Jones, Prof. Marcus E., Salt Lake City, Utah (40). 1893. **G**
Jordan, Prof. David S., Palo Alto, Menlo Park P. O., Cal. (31). 1883. **F**
Julien, A. A., New York Academy of Sciences, New York, N. Y. (24). 1875. **E C**

Kastle, Prof. J. H., State College of Kentucky, Lexington, Ky. (45). 1898. **C**
Kedzie, Prof. Nellie S., Peoria, Ill. (34). 1890. **I F**
Kedzie, Prof. Robert C., Agricultural College, Mich. (29). 1881. **C**
Keep, Wm. J., Detroit, Mich. (37). 1897.
Kellerman, Prof. William A., Ohio State University, Columbus, Ohio (41). 1893. **G**
Kemp, James F., School of Mines, Columbia University, New York, N. Y. (36). 1888. **E**
Kendall, Prof. E. Otis, 3826 Locust St., Philadelphia, Pa. (29). 1882. **A**
Kendrick, Prof. Arthur, Rose Polytechnic Institute, Terre Haute, Ind. (45). 1897. **B**
Kent, William, Passaic, N. J. (26). 1881. **D I**
Kenyon, Frederick C., Dep't of Agric., Washington, D. C. (46). 1897. **F**
Kershner, Prof. Jefferson E., Lancaster City, Pa. (29). 1883. **A B**
Kinealy, John H., Washington University, St. Louis, Mo. (36). 1891. **D**
King, F. H., Experiment Station, Madison, Wis. (32). 1892. **E F**
Kinnicutt, Dr. Leonard P., Polytechnic Institute, Worcester, Mass. (28). 1883. **C**
Klotz, Otto Julius, 437 Albert St., Ottawa, Ontario, Can. (38). 1889.
Kober, Geo. Martin, M.D., 1819 Q St. N. W., Washington, D. C. (40). 1896. **H**

Kunz, G. F., care Messrs. Tiffany & Co., Union Square, New York, N. Y. (29). 1883. **E H C**

Lacoe, Ralph D., Pittston, Pa. (31). 1893. **E F**
Ladd, Prof. E. F., Agricultural College, Fargo, No. Dakota (36). 1889. **C**
Laflamme, Prof. J. C. K., Laval University, Quebec, Can. (29). 1887. **E B**
LaFlesche, Francis, Indian Bureau, Interior Dep't, Washington, D. C. (33). 1885. **H**
Lamb, Daniel S., M.D., 800 10th St., N. W., Washington, D. C. (40). 1894. **H**
Lambert, Preston A., 215 S. Center St., Bethlehem, Pa. (41). 1896. **A**
Landreth, Olin H., Prof. of Civil Engineering, Union College, Schenectady, N. Y. (28). 1883. **D**
Langenbeck, Karl, 27 Orchard St., Zanesville, Ohio (39). 1896. **C**
Langley, Prof. J. W., Case School of Applied Science, Cleveland, Ohio (23). 1875. **C B**
Langley, Prof. S. P., Secretary Smithsonian Institution, Washington, D. C. (18). 1874. **A B**
Lanza, Prof. Gaetano, Massachusetts Institute of Technology, Back Bay, Boston, Mass. (29). 1882. **D A B**
Lattimore, Prof. S. A., University of Rochester, Rochester, N. Y. (15). 1874. **C**
Laudy, Louis H., Ph.D., School of Mines, Columbia University, New York, N. Y. (28). 1890. **C**
Lazenby, Prof. Wm. R., Columbus, Ohio (30). 1882. **B I**
Leach, Miss Mary F., Mt. Holyoke College, South Hadley, Mass. (44). 1896. **C**
LeBrun, Mrs. Michel, 8 Mountain Ave., Montclair, New Jersey (35). 1892. **F**
LeConte, Prof. Joseph, University of California, Berkeley, Cal. (29). 1881. **E F**
Ledoux, Albert R., Ph.D., 9 Cliff St., New York, N. Y. (26). 1881. **C**
Lefavour, Prof. Henry, Williams College, Williamstown, Mass. (42). 1894.
Lehmann, G. W., Ph.D., 412 E. Lombard St., Baltimore, Md. (30). 1885. **C B**
Lennon, William H., Brockport, N. Y. (31). 1894. **G C**
Lesley, Prof. J. Peter, P. O. Box 93, Milton, Mass. (2). 1874. **E**
Leverett, Frank, Denmark, Iowa (37). 1891. **E**
Libbey, Prof. William, Princeton, N. J. (29). 1887. **E F**
Lindenkohl, Adolphus, U. S. Coast and Geodetic Survey, Washington, D. C. (40). 1898. **E**
Lindenthal, Gustav, C.E., 45 Cedar St., New York, N. Y. (37). 1891. **I**
Livermore, Wm. R., Maj. of Eng. U. S. A., Care War Department, Washington, D. C. (38). 1895. **C H**
Lloyd, John Uri, Pharmaceutical Chemist, Court and Plum Sts., Cincinnati, Ohio (38). 1890. **C F**
Loeb, Morris, Ph.D., 118 West 72d St., New York, N. Y. (36). 1889. **C**
Lord, Prof. Nat. W., State University, Columbus, Ohio (29). 1881. **C**

LOUBAT, LE DUC DE, 47 rue Dumont d'Urville, Paris, France (46). 1897. **H**
Loud, Prof. Frank H., 1203 N. Tejon St., Colorado Springs, Colo. (29). 1890. **A B**
Loughridge, Dr. R. H., Ass't Prof. Agric. Chemistry and Agric. Geology, University of California, Berkeley, Cal. (21). 1874. **E C**
Love, Edward G., 80 E. 55th St., New York, N. Y. (24). 1882. **C**
Low, Seth, President Columbia University, New York, N. Y. (29). 1890.
Lowell, Augustus, 53 State St., Boston, Mass. (29). 1898.
Lowell, Percival, 53 State St., Boston, Mass. (36). 1896. **A**
Ludlow, Lt. Col. William, Corps of Eng. U. S. A., Army Building, New York, N. Y. (33). 1897. **D B**
Lyford, Edwin F., Springfield, Mass. (33). 1896. **B C H**
Lyle, David Alexander, Captain Ordnance Dept. U. S. A., Ordnance Office, War Department, Washington, D. C. (28). 1880. **D**
Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18). 1874.

McClintock, Emory, Morristown, N. J. (43). 1895. **A**
McCreath, Andrew S., 223 Market St., Harrisburg, Pa. (33). 1889. **C E**
McCurdy, Chas. W., Ph.D., Prof. of Chemistry, University of Idaho, Moscow, Idaho (35). 1895. **F E**
McDonnell, Prof. Henry B., College Park, Md. (40). 1893. **C**
MacDougal, Daniel T., University of Minnesota, Minneapolis, Minn. (44). 1897. **G**
McGee, Dr. Anita Newcomb, Bureau of American Ethnology, Washington, D. C. (37). 1892. **H**
McGee, W J, Bureau of American Ethnology, Washington, D. C. (27). 1882. **H E**
McGill, John T., Ph.D., Vanderbilt Univ., Nashville, Tenn. (36). 1888. **C**
McGregory, Prof. J. F., Colgate Univ., Hamilton, N. Y. (35). 1892. **C**
McMahon, James, Ithaca, N. Y. (36). 1891. **A**
MacMillan, Prof. Conway, University of Minnesota, Minneapolis, Minn. (42). 1894. **G**
McMurtrie, William, 100 William St., New York, N. Y. (22). 1874. **C**
McNeill, Malcolm, Lake Forest, Ill. (32). 1885. **A**
Mabery, Prof. C. F., Case School of Applied Science, Cleveland, Ohio (29). 1881. **C**
Macbride, Prof. Thomas H., Iowa City, Iowa (38). 1890. **G**
Macfarlane, Prof. A., Lehigh University, S. Bethlehem, Pa. (34). 1886. **B A**
Macloskie, Prof. George, Princeton University, Princeton, N. J. (25). 1882. **F G**
Magie, Prof. Wm. F., Princeton University, Princeton, N. J. (35). 1887.
MANN, B. PICKMAN, 1918 Sunderland Place, Washington, D. C. (22). 1874. **I F**
Marcy, Oliver, LL.D., Evanston, Ill. (10). 1874. **E**
Marindin, Henry Louis, U. S. Coast and Geodetic Survey, Washington, D. C. (40). 1898. **E**

Marlatt, Charles L., 1st Ass't Entomologist, Department of Agriculture, Washington, D. C. (40). 1895. **F**

Marsh, Prof. C. Dwight, Ripon, Wis. (34). 1893. **F E**

MARSH, PROF. O. C., Yale University, New Haven, Conn. (15). 1874. **F H**

Martin, Artemas, U. S. Coast Survey, Washington, D. C. (38). 1890. **A**

Martin, Prof. Daniel S., 126 Macon St., Brooklyn, N. Y. (23). 1879. **E F**

Marvin, C. F., U. S. Weather Bureau, Washington, D. C. (39). 1892. **B**

Marvin, Frank O., University of Kansas, Lawrence, Kan. (35). 1894. **D**

Mason, Prof. Otis T., National Museum, Washington, D. C. (25). 1877. **H**

Mason, Dr. William P., Prof. Rensselaer Polytechnic Institute, Troy, N. Y. (31). 1886. **C**

Matthews, Dr. Washington, 21 South Ingalls St., Ann Arbor, Mich. (37). 1888. **H**

Meehan, Thomas, Germantown, Pa. (17). 1875 **G**

Mees, Prof. Carl Leo, Rose Polytechnic Institute, Terre Haute, Ind. (24). 1876. **B C**

Mell, Prof. P. H., Polytechnic Institute, Auburn, Ala. (39). 1895. **E G**

Mendenhall, Prof. T. C., President Worcester Polytechnic Institute, Worcester, Mass. (20). 1874. **B**

Mercer, H. C., Doylestown, Bucks Co., Pa. (41). 1893. **H**

MERRILL, FREDERICK J. H., Ph.D., Ass't Director New York State Museum, Albany, N. Y. (35). 1887. **E**

Merriman, C. C., 1910 Surf St., Lake View, Chicago, Ill. (29). 1880. **F**

Merriman, Prof. Mansfield, S. Bethlehem, Pa. (32). 1885. **A D**

Merritt, Ernest, Ithaca, N. Y. (33). 1890. **B**

Metcalf, William, Pittsburgh, Pa. (33). 1894. **D**

Michael, Mrs. Helen Abbott, 44 Mount Vernon St., Boston, Mass. (33). 1885. **C F**

Miller, Prof. Arthur M., State College, Lexington, Ky. (45). 1898. **E**

Miller, Prof. William S., University of Wisconsin, Madison, Wis. (42). 1894. **F**

Mills, James, M.A., Guelph, Ontario, Can. (31). 1895. **I C**

Mills, Prof. Wesley, McGill College, Montreal, P. Q., Can. (31). 1886. **F H**

Minot, Dr. Charles Sedgwick, Harvard Medical School, Back Bay, Boston, Mass. (28). 1880. **F**

Minot, Francis, M.D., Readville, Mass. (29). 1884.

Mohr, Dr. Charles, Mobile, Ala. (40). 1895. **G**

Moler, Geo. S., 106 University Ave., Ithaca, N. Y. (38). 1892.

Moody, Robert O., M.D., Fair Haven Heights, New Haven, Conn. (35). 1892. **F**

Moore, Clarence B., 1321 Locust St., Philadelphia, Pa. (44). 1897. **H**

Moore, Prof. J. W., M.D., Lafayette College, Easton, Pa. (22). 1874. **B D A**

Moore, Prof. Willis L., Chief of Weather Bureau, Dep't Agriculture, Washington, D. C. (44). 1897. **B**

Moorehead, Warren K., Ohio State Univ., Columbus, Ohio (38). 1890. **H**

Moreland, Prof. S. T., Lexington, Va. (33). 1894. **B D**

Morley, Prof. Edward W., 23 Cutler St., Cleveland, Ohio (18). 1876. **C B E**

Morse, Prof. E. S., Salem, Mass. (18). 1874. **F H**
Morton, H., Stevens Institute Technology, Hoboken, N. J. (18). 1875. **B C**
Moser, Lieut. Comd'r Jeff. F., U. S. N., Com'dg U. S. F. S. Str. Albatross, Navy Pay Office, San Francisco, Cal. (28). 1889. **E**
Moses, Prof. Thomas F., Urbana University, Urbana, Ohio (25). 1883. **H F**
Munroe, Prof. C. E., Columbian Univ., Washington, D. C. (22). 1874. **C**
Munsterberg, Hugo, Prof. of Psychology, Harvard University, Cambridge, Mass. (47). 1898.
Murdoch, John, Public Library, Boston, Mass. (29). 1886. **F H**
Myers, John A., P. O. Box No. 1409, New York, N. Y. (30). 1889. **C**
Myers, William S., M.Sc., F.C.S., Assoc. Prof. of Chemistry, Rutgers College, New Brunswick, N. J. (43). 1898. **C**

Nagle, Prof. James C., A. and M. College, College Station, Texas (40). 1893. **D B**
Nardroff, Ernest R. von, 360a Tompkins Ave., Brooklyn, N. Y. (44). 1896. **B**
Nef, J. U., University of Chicago, Chicago, Ill. (39). 1891. **C**
Nelson, Prof. A. B., Centre College, Danville, Ky. (30). 1882. **A B D**
Newcomb, Harry T, Division of Statistics, Department of Agriculture, Washington, D. C. (47). 1898.
Newcomb, Prof. S., Navy Dep't, Washington, D. C. (13). 1874. **A B**
Newcombe, Frederick Chas., 51 E. Liberty St., Ann Arbor, Mich. (43). 1896. **G**
Newell, F. H., U. S. Geological Survey, Washington, D. C. (40). 1893.
Newell, William Wells, Editor Journal American Folk Lore, Cambridge, Mass. (41). 1893. **H**
Nichols, Ernest Fox, Hamilton, N. Y. (41). 1893. **B**
Nichols, E. L., Ph.D., Cornell University, Ithaca, N. Y. (28). 1881. **B C**
Niles, Prof. W. H., Cambridge, Mass. (16). 1874. **E**
Nipher, Prof. F. E., Washington University, St. Louis, Mo. (24). 1876. **B**
Nolan, Edw. J., M.D., Academy of Natural Sciences, Philadelphia, Pa. (29). 1890. **F**
NORTON, PROF. THOMAS H., University of Cincinnati, Cincinnati, Ohio (35). 1887. **C**
Novy, Dr. Frederick G., University of Michigan, Ann Arbor, Mich. (36). 1889. **C**
Noyes, Prof. Arthur A., Massachusetts Institute Technology, Back Bay, Boston, Mass. (45). 1897. **C**
Noyes, Miss Mary C., Ph.D., Lake Erie College, Painesville, Ohio (43). 1896. **B**
Noyes, Prof. Wm. A., Rose Polytechnic Institute, Terre Haute, Ind. (32). 1885. **C**
Nuttall, Mrs. Zella, care Peabody Museum, Cambridge, Mass. (35). 1887. **H**
Nutting, Prof. Charles C., State University of Iowa, Iowa City, Iowa (40). 1892. **F**

Oberholser, Harry Church, Division Biological Survey, Dep't Agriculture, Washington, D. C. (46). 1898. **C**

Ogden, Herbert G., U. S. C. and G. Survey, Washington, D. C. (38). 1891. **E**

Ordway, Prof. John M., Tulane University, New Orleans, La. (9). 1875. **C**

Orleman, Miss Daisy M., M.D., Peekskill Military Acad., Peekskill, N. Y. (40). 1897. **F**

Orr, William, jr., 30 Firglade Ave., Springfield, Mass. (39). 1895. **F B**

Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19). 1875. **E**

Osborn, Henry F., Columbia University, New York, N. Y. (29). 1883. **F**

Osborn, Herbert, Ohio State University, Columbus, Ohio (32). 1884. **F**

Osmond, Prof. I. Thornton, State College, Centre Co., Pa. (33). 1889. **B A C**

Packard, Dr. A. S., 115 Angell St., Providence, R. I. (16). 1875. **F E**

Paine, Cyrus F., 806 Granite Building, Rochester, N. Y. (12). 1874. **B A**

Palache, Charles, University Museum, Cambridge, Mass. (44). 1896. **E**

Pammel, Prof. L. H., Iowa Agricultural College, Ames, Iowa (39). 1892. **G**

Parke, John G., General U. S. A., 16 Lafayette Square, Washington, D. C. (29). 1881. **D**

PARKHURST, HENRY M., 178 Gates Ave., Brooklyn, N. Y. (23). 1874. **A**

Parks, C. Wellman, Civil Eng., U. S. N., care Bureau of Navigation, Washington, D. C. (42). 1897.

Parsons, Prof. C. Lathrop, Durham, N. H. (41). 1896.

Patrick, Geo. E., Ames, Iowa (36). 1890. **C**

Patterson, Geo. W., jr., Ann Arbor, Mich. (44). 1896.

Patterson, Harry J., College Park, Prince George's Co., Md. (36). 1890. **C**

Paul, Prof. Henry M., U. S. Naval Observatory, Washington, D. C. (33). 1885. **A B**

Peet, Rev. Stephen D., 5327 Madison Ave., Chicago, Ill. (24). 1881. **H**

Peirce, Prof. Benjamin O., 51 Oxford St., Cambridge, Mass. (47). 1898.

Peirce, George James, Stanford University, Cal. (44). 1897. **G**

PENROSE, Dr. R. A. F., Jr., Ph.D., 460 Bullitt Building, Philadelphia, Pa. (38). 1890. **E**

Perkins, Prof. George H., Burlington, Vt. (17). 1882. **H F E**

Perry, Arthur C., 226 Halsey St., Brooklyn, N. Y. (43). 1896. **A B**

Peters, Edw. T., P. O. Box 265, Washington, D. C. (33). 1889. **I**

Pettee, Prof. Wm. H., 554 Thompson St., Ann Arbor, Mich. (24). 1875. **E**

Phillips, Prof. A. W., New Haven, Conn. (24). 1879.

Phillips, Prof. Francis C., Western University, Allegheny, Pa. (36). 1889. **C**

Phillips, Dr. Wm. A., Evanston, Ill. (41). 1895. **H**

Pickering, Prof. E. C., Director of Harvard Observatory, Cambridge, Mass. (18). 1875. **A B**

Pierce, Perry Benjamin, U. S. Patent Office, Washington, D. C. (40). 1895. **H**

Pillsbury, J. E., Lieut. U. S. N., care Dr. W. L. Richardson, 225 Commonwealth Ave., Boston, Mass. (33). 1898. **E B**

Pillsbury, Prof. John H., Stoneham, Mass. (23). 1885. **F H**

Platt, Franklin, Ass't Geologist, 2nd Geological Survey of Pennsylvania, 1617 Chestnut St., Philadelphia, Pa. (27). 1882. **E**

Pohlman, Dr. Julius, Buffalo, N. Y. (32). 1884. **E F**

Powell, Major J. W., Washington, D. C. (23). 1875. **E H**

Power, Frederick B., Ph.D., care Messrs. Burroughs, Wellcome & Co., 42 Snow Hill, London, E. C., England (31). 1887. **C**

Prentiss, D. Webster, M.D., 1101 14th St., N. W., Washington, D. C. (29). 1882. **F**

Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1875. **C**

Pritchett, Henry S., Sup't U. S. Coast and Geodetic Survey, Washington, D. C. (29). 1881. **A**

Prosser, Charles S., Prof. of Geology, Union College, Schenectady, N. Y. (33). 1891. **E F**

Pulsifer, Wm. H., Newton Centre, Mass. (26). 1879. **A H**

Pupin, Dr. M. I., Columbia University, New York, N. Y. (44). 1896. **B**

Putnam, Prof. F. W., Curator Peabody Museum American Archæology and Ethnology, Cambridge, Mass.; Curator Dept. Anthropology, Amer. Museum Nat. History, Central Park, New York, N. Y. (10). 1874. **H**

Rathbun, Richard, Smithsonian Institution, Washington, D. C. (40). 1892. **F**

Raymond, Rossiter W., 13 Burling Slip, New York, N. Y. (15). 1875. **E I**

Raymond, Prof. Wm. G., Rensselaer Polytechnic Institute, Troy, N. Y. (44) 1896. **D**

Rees, Prof. John K., Columbia Univ., New York, N. Y. (26). 1878. **A E B**

Reese, Charles L., 1801 Linden Ave., Baltimore, Md. (39). 1892. **C**

Reese, Jacob, 400 Chestnut St., Philadelphia, Pa. (33). 1891. **D B**

Reid, Harry Fielding, Johns Hopkins Univ., Baltimore, Md. (36). 1893. **B**

Remsen, Prof. Ira, Johns Hopkins Univ., Baltimore, Md. (22). 1875. **C**

Reuter, Dr. Ludwig, Chief Chemist of Merck & Co., Merck Building, University Place, New York, N. Y. (46). 1898. **C**

Rice, Prof. Wm. North, Wesleyan University, Middletown, Conn. (18). 1874. **E F**

Richards, Prof. Charles B., 137 Edwards St., New Haven, Conn. (33). 1885. **D**

Richards, Edgar, 341 W. 88th St., New York, N. Y. (31). 1886. **C**

Richards, Prof. Robert H., Massachusetts Institute Technology, Back Bay, Boston, Mass. (22). 1875. **D**

Richards, Mrs. Robert H., Massachusetts Institute of Technology, Back Bay, Boston, Mass. (23). 1878. **C**

Richardson, Clifford, Sup't of Tests, Barber Asphalt Paving Co., Long Island City, N. Y. (30). 1884. **C**

Ries, Heinrich, Ph.B., Columbia University, New York, N. Y. (41). 1898. **E**

Ricketts, Prof. Palmer C., 17 1st St., Troy, N. Y. (33). 1887. **D A**

Ricketts, Prof. Pierre de Peyster, 104 John St., New York, N. Y. (26). 1880. **C D E**

Ripley, William Z., Ph.D., Newton Centre, Mass. (44). 1897. **H I**

Robinson, Benjamin Lincoln, Curator Harvard Herbarium, Cambridge, Mass. (41). 1893. **G**

Robinson, Prof. Franklin C., Bowdoin College, Brunswick, Me. (29). 1889. **C D**

Robinson, Prof. S. W., 1353 Highland St., Columbus, Ohio (30). 1883. **D B A**

Rockwell, Gen. Alfred P., Manchester, Mass. (10). 1882. **E**

Rockwell, Chas. H., Box 293, Tarrytown, N. Y. (28). 1883. **A D**

Rockwood, Prof. Charles G., jr., Princeton College, Princeton, N. J. (20). 1874. **A E B D**

Rominger, Dr. Carl, Ann Arbor, Mich. (21). 1879. **E**

Rood, Prof. O. N., Columbia University, New York, N. Y. (14). 1875. **B**

Rosa, Edward Bennett, Prof. of Physics, Wesleyan University, Middletown, Conn. (39). 1892. **A B**

Ross, Waldo O., 1 Chestnut St., Boston, Mass. (29). 1882.

Rotch, A. Lawrence, Director of Blue Hill Meteorological Observatory, Hyde Park, Mass. (39). 1896. **B**

Rowland, Prof. Henry A., Baltimore, Md. (29). 1880. **B**

Rowlee, W. W., Cornell University, Ithaca, N. Y. (41). 1894. **G**

Runkle, Prof. J. D., Massachusetts Institute of Technology, Boston, Mass. (2). 1875. **A D**

Rusby, Henry H., M.D., College of Pharmacy, 211 E. 23d St., New York, N. Y. (36). 1890. **G**

Russell, Prof. H. L., University of Wisconsin, Madison, Wis. (41). 1894. **G**

Russell, I. C., University of Mich., Ann Arbor, Mich. (25). 1882. **E**

Ryan, Harris J., Cornell University, Ithaca, N. Y. (38). 1890. **B**

Sadtler, Sam'l P., 1042 Drexel Building, Philadelphia, Pa. (22). 1875. **C**

Safford, Dr. James M., Nashville, Tenn. (6). 1875. **E C F**

Safford, Prof. Truman H., Williamstown, Mass. (41). 1892. **A**

Salisbury, Prof. R. D., Chicago University, Chicago, Ill. (37). 1890. **B E**

Saunders, Prof. Charles E., 32 St. Mary St., Toronto, Ontario, Can. (41). 1895. **C**

Saunders, William, LL.D., F.R.S.C., F.L.S., Director Canadian Experimental Farms, Ottawa, Can. (17). 1874. **F**

Saville, Marshall H., American Museum Natural History, Central Park, New York, N. Y. (39). 1892. **H**

Schaeberle, J. M., Astronomer in the Lick Observatory, San José, Cal. (34). 1886. **A**

Schanck, Prof. J. Stillwell, Princeton, New Jersey (4). 1882. **C B H**

Schott, Charles A., U. S. Coast and Geodetic Survey Office, Washington, D. C. (8). 1874. **A**

Schwarz, E. A., 230 New Jersey Ave., Washington, D. C. (29). 1895. **F**

Schweinitz, Dr. E. A. de, Department of Agriculture, Washington, D. C. (36). 1889. **C**

Schweitzer, Prof. Paul, State University of Missouri, Columbia, Mo. (24). 1877. **C B**

SCUDDER, SAMUEL H., Cambridge, Mass. (13). 1874. **F**

Scull, Miss S. A., Smethport, McKean Co., Pa. (40). 1895. **H**

Seaman, W. H., Chemist, 1424 11th St. N. W., Washington, D. C. (23). 1874. **C F**

Searle, Prof. Geo. M., Catholic University, Washington, D. C. (39). 1891. **A**

Sedgwick, Prof. William Thompson, Mass. Institute Technology, Back Bay, Boston, Mass. (47). 1898. **F G**

See, Horace, 1 Broadway, New York, N. Y. (34). 1886. **D**

Seymour, Arthur Bliss, Cambridge, Mass. (36). 1890. **G**

Seymour, Paul Henry, 479 Second Ave., Detroit, Mich. (44). 1896. **C**

Sharp, Dr. Clayton H., Ithaca, N. Y. (45). 1897.

Sharples, Stephen P., 13 Broad St., Boston, Mass. (29). 1884. **C**

Shaw, Prof. James Byrnie, 1030 Grove St., Jacksonville, Ill. (43). 1896. **A**

Sheldon, Samuel, A.M., Ph.D., Polytechnic Institute, Brooklyn, N. Y. (42). 1894. **B**

Shelton, Prof. Edward M., Department of Agriculture, Brisbane, Queensland, Australia (32). 1892. **F**

Shimer, Porter W., E.M., Easton, Pa. (38). 1889. **C**

Shufeldt, Dr. R. W., 2508 University Place, N. W., Washington, D.C., (40). 1892. **F**

Shutt, Frank T., M. A., F. E. C., F. C. S., Chief Chemist Canadian Experimental Farm, Ottawa, Ontario, Can. (47). 1898. **C**

Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10). 1874.

Sigsbee, Chas. D., Com'd'r U. S. N., U. S. Hydrographic Office, Washington, D. C. (28). 1882. **D E**

Simon, Dr. Wm., 1348 Block St., Baltimore, Md. (29). 1895. **C**

Simonds, Prof. Frederic W., University of Texas, Austin, Texas (25). 1888. **E F**

Skilton, James A., 115 Broadway, New York, N. Y. (43). 1895. **I**

Skinner, Aaron N., U. S. Naval Observatory, Washington, D. C. (40). 1893. **A**

Smith, Alex., Ph.D., University of Chicago, Chicago, Ill. (40). 1892. **C**

Smith, Prof. Chas. J., 35 Adelbert St., Cleveland, Ohio (32). 1885. **A B**

Smith, Prof. Edgar F., University of Pennsylvania, Philadelphia, Pa. (33). 1891. **C**

Smith, Edwin, Rockville, Montgomery Co., Md. (30). 1882. **A B**

Smith, Prof. Erastus G., Beloit College, Beloit, Wis. (34). 1887. **C**

Smith, Ernest Ellsworth, 262 Fifth Ave., New York, N. Y. (43). 1898.

Smith, Erwin F., Department of Agriculture, Washington, D. C. (34). 1890. **G**

Smith, Prof. Eugene A., University, Ala. (20). 1877. **E C**

Smith, Harlan I., American Museum Natural History, Central Park, New York, N. Y. (41). 1896. **H**

Smith, James Perrin, Ph.D., Ass't Prof. of Paleontology, Leland Stanford Junior University, Palo Alto, Cal. (37). 1894. **C E**

Smith, John B., Professor of Entomology, Rutgers College, New Brunswick, N. J. (32). 1884. **F**

SMITH, QUINTIUS C., M.D., 617 Colorado St., Austin, Texas (26). 1881. **F**

Smock, Prof. John Conover, Trenton, N. J. (23). 1879. **E**

Smyth, C. H., jr., Clinton, N. Y. (38). 1894. **E**

Snow, Prof. F. H., Lawrence, Kan. (29). 1881. **F E**

Snyder, Prof. Harry, Saint Anthony Park, Minn. (44). 1897. **C**

Snyder, Prof. Monroe B., High School Observatory, Philadelphia, Pa. (24). 1882. **A B**

Soule, R. H., Baldwin Locomotive Works, 1217 Monadnock Building, Chicago, Ill. (33). 1886. **D**

Spencer, Prof. J. William, 1710 Q St., Washington, D. C. (28). 1882. **E**

SPENZER, JOHN G., M.D., 370 Central Avenue, Cleveland, Ohio (37). 1895. **C**

Spinney, L. B., Iowa State College, Ames, Iowa (42). 1897. **B**

Springer, Dr. Alfred, Box 621, Cincinnati, Ohio (24). 1880. **C**

Squibb, Edward R., M.D., 152 Columbia Heights, Brooklyn, N. Y. (43). 1896.

Starr, Frederick, Ph.D., Prof. University of Chicago, Chicago, Ill. (36). 1892. **H E**

Stearns, Robert E. C., Ph.D , 1025 East Eighteenth St., Los Angeles, Cal. (18). 1874. **F**

Steinmetz, Chas. Proteus, General Electric Co., Schenectady, N. Y. (40). 1895. **B**

STEPHENS, W. HUDSON, Lowville, N. Y. (18). 1874. **E H**

Sternberg, George M., M.D., LL.D., Surgeon General U. S. A., War Department, Washington, D. C. (24). 1880. **F**

Stevens, Prof. W. LeConte, Rensselaer Polytechnic Institute, Troy, N. Y. (29). 1882. **B**

Stevenson, Prof. John J., Univ. Heights, New York, N. Y. (36). 1888. **E**

Stevenson, Mrs. Matilda C., Bureau of Ethnology, Washington, D. C. (41). 1893. **H**

Stieglitz, Dr. Julius, University of Chicago, Chicago, Ill. (39). 1895. **C**

Stokes, Henry Newlin, Ph.D., U. S. Geological Survey, Washington, D. C. (38). 1891. **C E**

Stone, Ormond, Director Leander McCormick Observatory, University of Virginia, Va. (24). 1876. **A**

Story, Prof. Wm. E., Clark University, Worcester, Mass. (29). 1881. **A**

Stowell, Prof. T. B., Potsdam, N. Y. (28). 1885. **F**

Stuart, Prof. A. P. S., Lincoln, Nebraska (21). 1874. **C**

Sturgis, Wm. C., 384 Whitney Ave., New Haven, Conn. (40). 1892. **G**

Sturtevant, E. Lewis, M.D., S. Framingham, Mass. (29). 1882. **G**

Swingle, Walter T., Department of Agriculture, Washington, D. C. (40). 1892. **G**

Tainter, Charles Sumner, Central Power Station, Washington, D. C. (29). 1881. **B D A**

Talbot, Henry P., Prof. Analytical Chemistry, Massachusetts Institute of Technology, Back Bay, Boston, Mass. (44). 1896. **C**

Taylor, Frank B., 391 Fairfield Ave., Fort Wayne, Ind. (39). 1897.

Tesla, Nikola, LL.D., 55 W. 27th St., New York, N. Y. (43). 1895. **B**

Thomas, Benjamin F., State University, Columbus, Ohio (29). 1882. **B A**

Thomas, Prof. M. B., Crawfordsville, Ind. (41). 1894. **G**

Thompson, Joseph Osgood, Amherst, Mass. (41). 1893.

Thomson, Elihu, Thomson-Houston Electric Co., Lynn, Mass. (37). 1888. **B**

Thomson, Wm., M.D., 1426 Walnut St., Philadelphia, Pa. (33). 1885. **B**

Thornburg, Charles L., Prof. Math. and Astron., Lehigh University, S. Bethlehem, Pa. (44). 1897. **A**

Thruston, Gates Phillips, Nashville, Tenn. (38). 1890. **H**

Thruston, R. C. Ballard, care Ballard & Ballard Co., Louisville, Ky. (36). 1896. **E**

Thurston, Prof. R. H., Sibley College, Cornell University, Ithaca, N. Y. (23). 1875. **D**

Tittmann, Otto H., U. S. Coast and Geodetic Survey Office, Washington, D. C. (24). 1888. **A**

Todd, Prof. David P., Director Lawrence Observatory, Amherst College, Amherst, Mass. (27). 1881. **A B D**

Todd, Prof. James E., Box 22, Vermillion, S. Dak. (22). 1886. **E F**

Tooker, William Wallace, Sag Harbor, N. Y. (43). 1895. **H**

Tracy, Sam'l M., Biloxi, Miss. (27). 1881. **G**

Traphagen, Frank W., Ph.D., Bozeman, Montana (35). 1889. **C F E**

Trelease, Dr. Wm., Director Missouri Botanical Gardens, St. Louis, Mo. (39). 1891. **G**

Trenholm, Hon. W. L., President American Surety Co., 160 Broadway, New York, N. Y. (35). 1896.

Trimble, Prof. Henry, 145 N. 10th St., Philadelphia, Pa. (34). 1889. **C**

True, Fred W., National Museum, Washington, D. C. (28). 1882. **F**

Tucker, Willis G., M.D., Albany Medical College, Albany, N. Y. (29). 1888. **C**

TUCKERMAN, ALFRED, Ph.D., 342 W. 57th St., New York, N. Y. (39). 1891. **C**

Tuttle, Prof. Albert H., University of Virginia, Charlottesville, Va. (17). 1874. **F**

Twitchell, E., 10 Bellevue Ave., Mt. Auburn, Cincinnati, Ohio (39). 1891. **C**

Uhler, Philip R., 254 W. Hoffman St., Baltimore, Md. (19). 1874. **F E**

Underwood, Lucien M., Prof. of Botany, Columbia University, New York, N. Y. (33). 1885. **G**

Upham, Warren, Librarian of the Minnesota Historical Society, St. Paul, Minn. (25). 1880. **E**

Upton, Winslow, Brown University, Providence, R. I. (29). 1883. **A**

Van Dyck, Prof. Francis Cuyler, New Brunswick, N. J. (28). 1882. **B C F**
Van Hise, Charles R., University of Wisconsin, Madison, Wis. (37). 1890.
Van Vleck, Prof. John M., Wesleyan University, Middletown, Conn. (23). 1875. **A**
Veeder, Major Albert, M.D., Lyons, Wayne Co., N. Y. (36). 1895.
Venable, Prof. F. P., Chapel Hill, N. C. (39). 1891. **C**
Verrill, Prof. A. E., New Haven, Conn. (47). 1898. **F**
Vogdes, A. W., Captain 5th Artillery, Fort Mason, San Francisco, Cal. (32). 1885. **E F**
Voorhees, Louis A., P. O. Box 290, New Brunswick, N. J. (43). 1895. **C**

Wadsworth, M. Edward, Ph.D., President Michigan College of Mines, and Prof. of Mineralogy, Petrography and Geology, Houghton, Mich. (23). 1874. **E**
Wagner, Frank C., Rose Polytechnic Institute, Terre Haute, Ind. (34). 1897. **D**
Waite, M. B., Dep't of Agriculture, Washington, D. C. (37). 1893. **G**
Walcott, Charles D., Director U. S. Geological Survey, Washington, D. C. (25). 1882. **E F**
Waldo, Prof. Clarence A., Purdue University, Lafayette, Ind. (37). 1889. **A**
Waldo, Leonard, S. D., 57 Coleman St., Bridgeport, Conn. (28). 1880. **A**
Wallace, Wm., Ansonia, Conn. (28). 1882.
WALLER, E., School of Mines, Columbia University, New York, N. Y. (23). 1874.
Ward, Prof. Henry A., Rochester, N. Y. (13). 1875. **F E H**
Ward, Lester F., U. S. Geological Survey, Washington, D. C. (26). 1879. **E G**
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874. **G F**
Ward, Wm. E., Port Chester, N. Y. (36). 1889. **D**
Warder, Prof. Robert B., Howard University, Washington, D. C. (19). 1881. **C B**
WARNER, JAMES D., 199 Baltic St., Brooklyn, N. Y. (18). 1874. **A B**
Warner, Worcester R., 1722 Euclid Ave., Cleveland, Ohio (33). 1888. **A B D**
Warren, Dr. Joseph W., Bryn Mawr Coll., Bryn Mawr, Pa. (31). 1886. **F**
Warren, Prof. S. Edward, Newton, Mass. (17). 1875. **A–I**
Washington, Dr. Henry S., Locust, N. J. (44). 1897. **E**
WATSON, PROF. WM., 107 Marlborough St., Boston, Mass. (12). 1884. **A**
Wead, Charles K., Patent Office, Washington, D. C. (47). 1898. **B**
Webb, Prof. J. Burkitt, Stevens Inst., Hoboken, N. J. (31). 1883. **D B A**
Weber, Prof. Henry A., Ohio State Univ., Columbus, Ohio (35). 1888. **F**
Webster, F. M., Wooster, Ohio (35). 1890. **F**
Webster, Prof. N. B., Vineland, N. J. (7). 1874. **B C E**
Weed, Clarence M., Durham, N. H. (38). 1890. **F**
Weld, Prof. Laenas G., State University of Iowa, Iowa City, Iowa (41). 1895. **A**
WEST, Dr. CHARLES E., Brooklyn, N. Y. (1). 1895.

Weston, Edward, 645 High St., Newark, N. J. (33) 1887. **B C D**
White, David, U. S. Geological Survey, Washington, D. C. (40). 1892. **E F**
White, Prof. H. C., University of Georgia, Athens, Ga. (29). 1885. **C**
White, Prof. I. C., State Geologist of West Virginia, Morgantown, W. Va. (25). 1882. **E**
Whiteaves, J. F., Geological Survey, Ottawa, Ontario, Can. (31). 1887. **E F**
Whitfield, J. Edward, 406 Locust St., Philadelphia, Pa. (44). 1896. **C**
Whitfield, R. P., American Museum Natural History, 77th St. and 8th Avenue, New York, N. Y. (18). 1874. **E F H**
Whiting, Miss Sarah F., Wellesley College, Wellesley, Mass. (31). 1883. **B A**
Whitman, Prof. Charles O., Chicago University, Chicago, Ill. (43). 1898. **F**
Whitman, Prof. Frank P., Adelbert College, Cleveland, Ohio (33). 1885. **A B**
Wiley, Prof. Harvey W., Department of Agriculture, Washington, D.C. (21). 1874. **C**
Williams, Benezette, 171 La Salle St., Chicago, Ill. (33). 1887. **D**
Williams, Charles H., M.D., 15 Arlington St., Boston, Mass. (22). 1874.
Williams, Prof. Edw. H., jr., 117 Church St., Bethlehem, Pa. (25). 1894. **E D**
Williams, Francis H., M.D., 505 Beacon St., Boston, Mass. (29). 1890.
Williams, Prof. Henry Shaler, Yale University, New Haven, Conn. (18). 1882. **E F**
Willis, Bailey, U. S. Geological Survey, Washington, D. C. (36). 1890.
Willoughby, Charles C., Peabody Museum, Cambridge, Mass. (45). 1897. **H**
Willson, Prof. Frederick N., Princeton, N. J. (33). 1887. **A D**
Willson, Robert W., Cambridge, Mass. (30). 1890. **B A**
Wilson, Joseph M., Room 1036, Drexel Building, Philadelphia, Pa. (33). 1886. **D**
Wilson, Robert N., Macleod, Alberta, Can. (42). 1895. **H**
Wilson, Thomas, National Museum, Washington, D. C. (36). 1888. **H**
Wilson, Prof. William Powell, Director, Philadelphia Museums, 233 S. Fourth St., Philadelphia, Pa. (38). 1889. **G**
Winchell, Horace V., 1306 S. E. 7th St., Minneapolis, Minn. (34). 1890. **E C**
Winchell, Prof. N. H., University of Minnesota, Minneapolis, Minn. (19). 1874. **E H**
Winterhalter, A. G., Lt. U. S. N., League Island, Navy Yard, Philadelphia, Pa. (37). 1893. **A**
Withers, Prof. W. A., Agricultural and Mechanical College, Raleigh, N. C. (33). 1891. **C**
Witthaus, Dr. R. A., 414 East 26th St., New York, N. Y. (35). 1890.
Wolff, Dr. J. E., 15 Story St., Cambridge, Mass. (36). 1894. **E**
Woll, Fritz Wilhelm, Madison, Wis. (42). 1897. **C**

Woodbury, C. J. H., Amer. Bell Telephone Co., 125 Milk St., Boston, Mass. (29). 1884. **D**

Woodman, Dr. Durand, 80 Beaver St., New York, N. Y. (41). 1896.

Woodrow, James, President South Carolina College, Columbia, S. C. (43). 1895. **E**

Woods, Albert F., Dep't of Agriculture, Washington, D. C. (43) 1897. **G**

Woodward, Prof. Calvin M., 1761 Missouri Ave., St. Louis, Mo. (32). 1884. **D A I**

Woodward, R. S., Columbia University, New York, N. Y. (33). 1885. **A B D**

Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24). 1880. **E F**

Wright, Prof. Arthur W., Yale University, New Haven, Conn. (14). 1874. **A B**

Wright, Carroll D., LL.D., Department of Labor, Washington, D. C. 1894. **I**

Wright, Rev. Geo. F., Oberlin College, Oberlin, Ohio (29). 1882. **E H**

Würtele, Rev. Louis C., Acton Vale, P. Q., Can. (11). 1875. **E**

Youmans, Wm. Jay, M.D., Popular Science Monthly, 72 Fifth Ave., New York, N. Y. (28). 1889. **F C**

Young, A. V. E., Northwestern University, Evanston, Ill. (33). 1886. **C B**

Young, C. A., Prof. of Astronomy, Princeton University, Princeton, N. J. (18). 1874. **A B D**

Zalinski, E. L., U. S. A., Century Club, 7 W. 43d St., New York, N. Y. (36). 1891. **D**

Ziwet, Alexander, 44 Madison St., Ann Arbor, Mich. (38). 1890. **A**

[777 HONORARY FELLOWS AND FELLOWS.]

SUMMARY.—PATRONS, 2; CORRESPONDING MEMBER, 1; MEMBERS, 949; HONORARY FELLOW, 1; FELLOWS, 776.

NOV. 3, 1896, TOTAL NUMBER OF MEMBERS OF THE ASSOCIATION, 1729.

DECEASED MEMBERS.

A list of deceased members of the Association, so far as known at the time of publishing the volume of Proceedings of the Springfield meeting, May 1896, is given in that volume. At the Buffalo meeting the Council directed the Permanent Secretary to omit the printing of the full list of deceased members in the annual volumes and to print only the additions to the list.

Since the publication of the list printed in the Detroit volume (46), notices have been received of the decease of the following members. This list also includes corrections of the list printed in the Detroit volume.

Bromwell, William, Port Deposit, Md. (40). Died in Port Deposit, Md. Sept., 1898.

Carpenter, William Lewis, Washington, D. C. (24). Born in Dunkirk, N. Y., Jan. 13, 1844. Died at Madison Barracks, N. Y., July 10, 1898.

Coon, Henry Clarke, Alfred, N. Y. (29). Born in West Edmiston, N. Y., Jan. 28, 1828. Died in Alfred, N. Y., May 9, 1898.

De Forest, Henry S., Talladega, Ala. (32).

Fuller, Andrew S., Ridgewood, N. J. (24). Born in Utica, N. Y., Aug. 3, 1828. Died in Ridgewood, N. J., May 4, 1896.

Griscom, William Woodnut, Haverford College P. O., Pa. (33). Born July 6, 1851. Died Sept. 24, 1897.

Hacker, William, Philadelphia, Pa. (33).

Hall, James, Albany, N. Y. (1) Born in Hingham, Mass., in 1811. Died in Bethlehem, N. H., Aug. 7, 1898.

Holley, George W., Ithaca, N. Y. (19). Born Feb. 17, 1810. Died June 12, 1897.

Humphrey, James Ellis, Baltimore, Md. (44). Died in Jamaica, Aug., 1897.

Johnson, Lorenzo N., Boulder, Col. (39). Died in Feb., 1897.

Kellicott, David S., Columbus, Ohio (31). Died April 13, 1898.

Lawrence, George N., New York, N. Y. (7). Born Oct. 20, 1806. Died Jan. 17, 1895.

Leuckart, Rudolf, Leipzig, Germany (44). Hon. fellow. Born in Braunschweig, Oct. 7, 1823. Died Feb. 7, 1898.

Lindsley, J. Berrien, Nashville, Tenn. (1). Founder. Born in Princeton, N. J., Oct. 24, 1822. Died in Nashville, Tenn., Oct. 7, 1897.

Lintner, Joseph Albert, Albany, N. Y. (22). Born in Schoharie, N. Y., Feb. 8. 1822. Died in Florence, Italy, May 6, 1898.

Macdougall, Alan, Toronto, Can. (38.) Born in India, May 22, 1842. Died at Exmouth, South Devon, April 23, 1897.

Malleson, Philip Arthur, New York, N. Y. (46). Born in Glens Falls, N. Y., Dec. 25, 1859. Died Sept. 18, 1898.

Merrick, Edwin T., New Orleans, La. (29). Died in New Orleans, La., Jan. 13, 1897, in his 87th year.

Miles, Manley, Lansing, Mich. (29). Born in Homer, N. Y., July 20, 1826. Died in Lansing, Mich., Feb. 15, 1898.

Molson, John H. R., Montreal, Can. (31). Born in Kingston, Ont., June 5, 1826. Died in Montreal, Quebec, May 28, 1897.

Potter, O. B., New York, N. Y. (36).

Price, J. Sergeant, Philadelphia, Pa. (33). Born in Philadelphia, Pa., June 10, 1831. Died at Cape May, N. J., Aug. 16, 1897.

Rice, William, Springfield, Mass. (44). Born in Springfield, Mass., March 10, 1821. Died in Springfield, Mass., Aug. 17, 1897.

Rogers, William Augustus, Waterville, Me. (15). Born in Waterford, Conn., Nov. 13, 1832. Died in Waterville, Me., March 1, 1898.

Thompson, Daniel G., New York, N. Y. (29). Died July 10, 1897.

Wilkinson, J. Henderson, Washington, D. C. (35). Died in Washington, D. C.. Jan. 11, 1898.

Wood, De Volson, Hoboken, N. J. (29). Born in Smyrna, N. Y. Died in Hoboken, N. J.

Worthen, W. E., New York, N. Y. (36).

ADDRESS

BY

WOLCOTT GIBBS,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

THE time-honored custom of our Association makes it incumbent upon the retiring President to deliver an address upon some subject connected, if possible, with his own work, and not purely elementary or historical, but with at least some fresh ideas and some new facts. The task is a difficult one for a chemist, for there is perhaps no branch of science in which, of late years, there has been so much mental activity, and it is hard to find any subject which has not been worn threadbare in discussion.

Trusting to your indulgence, I will here present some theoretical points connected in part with my own work, and will treat them as briefly as the nature of the subjects will permit.

All chemists are familiar with the terms atom and molecule. The use of these two words, with a clear conception of their meaning, forms an era in the history of the science. Our modern chemistry is built up of atoms and molecules, as we now define them. Our modern physics deals for the most part and, as I think, too exclusively with atoms; except, perhaps, in the case of what we now term physical chemistry, the new branch of science which makes it difficult for us to determine where chemistry leaves off and where physics begins. The old controversy between the advocates of the continuity and those of the discontinuity of matter is not dead but only sleeps.

It must be admitted that the conception of the atom, as at present received, is not without serious objections, and probably many accept it as presenting fewer difficulties than the theory of continuity. I venture to suggest that the idea of a material limit, like that of space or quantity in mathematics, avoids at least some

difficulties, so that we may define an atom as a limit to which we approach as we subdivide a given mass of any element, having reference to properties alone. Or, taking a somewhat different mathematical conception, we may consider atoms as material differentials of which the general integrals which we term masses are made up, and in this way we may connect purely material with purely mathematical conceptions. The conception of a limit or of a differential avoids all hypotheses as regards form as well as regards magnitude, and retains only ideas of mass and kind. Conversely, I doubt whether, for the purpose of instruction in the calculus, any purely mathematical conception of limits or of differentials and integrals gives as clear an idea of these as does the comparison with physical or chemical atoms and their aggregates.

As all chemists know, we owe to Dalton the first clear conception of the chemical atom as distinguished from the atoms of Leucippus and Democritus; to Frankland the conception of valence which shows us what combinations of atoms can exist consistently with the number of units of affinity possessed by each individual atom; or, in other words, in what manner groups of atoms can form systems which are in stable equilibrium. This conception includes that of the chemistry of space, now so much employed in organic chemistry, as well as that of the motions of atoms within the molecule as yet almost without supporters. To the conception of a definite number of units of affinity, recent chemistry has added that of direction of action, statical according to one school of chemists, dynamical according to another.

Within a comparatively short time attention has been directed to a large class of compounds exhibiting very interesting properties and forming peculiar series, some of which, at least, are made up of homologous terms. This group has been called complex-inorganic, because many of its members form highly complex molecules of which no analogues are known.

I may here remark that Ostwald uses the term "complex" to denote only those molecules or groups of associated atoms which are unitary and do not belong to the class of double salts. I have not drawn this distinction in the present paper, because it is not that which was originally proposed, and because at present we are not able to separate the two classes from each other, except in a very limited number of cases.

No one class of compounds appears so well adapted to throw light upon the structure and modes of combination of molecules.

For this reason I have selected the group as the subject of my address to-day.

In endeavoring to establish a plausible theory of the constitution of the complex-inorganic acids and their salts it will be advantageous to state first succinctly the principal facts to be explained. These, I think, are as follows:

1. The teroxides of molybdenum and tungsten possess, in a remarkable degree, what may be termed a "cumulative" character, each being capable of forming series of salts which contain a very large relative number of molecules of the teroxide. In this respect they stand alone, so far at least as our present knowledge extends. Certain other oxides share in this peculiarity to a much less degree, as, for example, chromic and perhaps uranic oxides.

I will cite, in the first place, the series of metatungstates, the existence of which has, I believe, been fully established, partly by the researches of other chemists and partly by those made in my own laboratory.

The first term in the series has the general formula $RO.4WO_3$; the highest appears to be represented by the formula

$$11\ RO.24WO_3.$$

These, with the intermediate compounds, form a series of homologous terms, the common difference being $RO.2WO_3$.

Molybdenum forms a similar, but, in the present state of our knowledge, not so fully represented series. Moreover, in this case, it seems necessary to assume that certain metamolybdates contain a greater or less number of molecules of water as base, either partly or wholly replacing fixed oxides. In my first paper I have discussed this subject, and may here refer to that paper for necessary details. A revision of the composition of the alkaline molybdates is much to be desired, taking specially into account the number of molecules of water retained at high temperatures.

I may here remark that my views in regard to the existence of special classes of metatungstates and metamolybdates have not only not found acceptance with other chemists, but, so far as I know, have never even attracted attention.

2. Two different metatungstates may unite to form a double salt, as, for instance,

$$12WO_3.5RO+10WO_3.4R'O+66H_2O$$
$$12WO_3.5Na_2O+10WO_3.4Na_2O+51H_2O.$$

Such compounds, however, are not numerous, and it is possible to explain their structure in other ways. Similar molybdates have not been observed, except in cases in which other oxides, as for instance platinic oxide, are present.

3. Metatungstates and metamolybdates may react with a great variety of other salts to form complex salts in which a relatively very large number of molecules of tungstic or molybdic oxides appear, as compared with the number of molecules of the combined salt or oxide.

4. Metatungstates or metamolybdates may unite with complex salts containing tungstic or molybdic oxides, as, for example,

$$10WO_3PtO_2 4Na_2O + 2\{10WO_3 4Na_2O\} + 72H_2O.$$

5. Complex acids in the molecule which is united with the tungstic or molybdic oxide appear not to lose their special characters and, in many cases, though not in all, to retain their basicity. Thus in the phosphotungstate $12WO_3P_2O_5H_6$, the phosphoric acid is still orthophosphoric acid. Two different oxides, acids or salts may unite with tungstic or molybdic oxide, each retaining its own character. I will cite only the phosphoroso-phosphotungstate

$$24WO_3 2PO_3H_3.P_2O_5 5K_2O + 13H_2O$$

and the arsenoso-phosphotungstate

$$32WO_3.14As_2O_3.3P_2O_5 10K_2O + 28H_2O.$$

Classes of salts have been described in which orthophosphoric, hypophosphorous, phosphorous, pyrophosphoric, monometaphosphoric and hexametaphosphoric molecules appear as such.

The first of the formulas above given may be written

$$12WO_3P_2O_5 2K_2O.H_2O + 12\,WO_3.2POK_3 + 15\,H_2O$$

and will represent a quadruple salt if each term is to be considered as representing a double salt.

6. In the great majority of cases the number of molecules of water in the meta-salts as well as in their derivatives is extraordinarily large, but in the present state of our knowledge we are unable to distinguish with certainty between water of constitution and water of crystallization.

In considering the theory of the complex inorganic acids it will

be most advantageous to begin with the group of silicotungstates discovered by Marignac. Of these there are four classes, two of which are isomeric. We may write the general formulas of the neutral salts as follows, omitting water of crystallization:

$10\ WO_3.SiO_2.4RO$	$10\ WO_3.SiO_2.2RO.2H_2O$
$11\ WO_3\ SiO_3.4RO$	
$12\ WO_3.SiO_2.4RO$	$12\ WO_3.SiO_2.2RO.2H_2O$
$12\ WO_3.SiO_2.4RO$	

It is, of course, possible that the second group, containing eleven molecules of tungstic oxide, may have twice the molecular mass given, so that the general formula is:

$$12\ WO_3.SiO_2.4RO+10\ WO_3.SiO_24RO$$

The potassic salt, which is the only one described by Marignac, will then be the analogue of the double sodium and zinc metatungstate, which I have described, and which has, at least probably, the formula:

$$12\ WO_3.5Na_2O+10WO_3.4ZnO$$

Taking now the first series or group, the general term may be written in one of several ways, as, for example:

(1) $10WO_3.SiO_2(RO)_4$
(2) $10WO_3.RO+SiO_2(RO)_3$
(3) $10WO_32RO+SiO_2(RO)_2$
(4) $10WO_33RO+SiO_2RO$
(5) $10WO_34RO+SiO_2$

Formulas (1) and (2) appear to be excluded by the fact that silicates of the forms $SiO_2(RO)_4$ and $SiO_2(RO)_3$ are not known to exist, the only forms of silicic acid occurring in silicates containing a single base, in other words, in single as distinguished from double salts, being metasilicic acid SiO_3H_2 and orthosilicic acid SiO_4H_4 or SiO_2H_2O and SiO_22H_2O or corresponding salts. On the other hand, formulas (3) and (4) are admissible so far as the silica is concerned, but considered as double salts they require us to assume the existence of $10\ WO_32RO$ and $10\ WO_33RO$, which are not known to exist. Formula (5) is also admissible, if we suppose that a molecule of water is present, and thus we have

$$10WO_3.4RO+SiO_2OH_2.$$

Marignac considered all the acids of this series as quadribasic and consequently that two molecules of basic water were present in each of a number of well-defined salts.

From the above it appears difficult to explain the constitution of the various silicotungstates and tungstosilicates if we consider them as simply double salts. We must assume either that tungstates not known to exist or that silicates which have no analogues or that both these classes enter together into the structure of the complex molecules.

We get no assistance from a consideration of the mode of preparation of the complex salts in question. Marignac obtained them by boiling gelatinous silica with metatungstates. Gelatinous silica has no definite constitution, but appears to be simply a mixture of two or more forms of silicic acid. It has not been shown, I believe, that soluble alkaline silicates dissolve tungstic oxide on boiling, but Parmentier found that silicomolybdates are formed by the action of alkaline silicates upon alkaline molybdates in presence of silicic acid, and Pechard has shown that a silicomolybdate of the twelve molecule series is formed by the action of fluosilicic acid upon the 14:6 metamolybdate of ammonium.

The same reasoning applies to the silicotungstates and tungstosilicates, which contain twelve molecules of tungstic oxide, since the last named classes of acids also unite with four molecules of base.

In this connection three remarkable double salts deserve notice. These have respectively the formulas:

$36WO_3.3SiO_2.2Al_2O_3 + 93H_2O$ and $+ 81H_2O$
$36WO_3.3SiO_2.2Al_2O_3.9(NH_4)_2O + 75H_2O$
$36WO_3.3SiO_2.2N_2O_5.8Na_2O + 45H_2O.$

These salts may all be considered as containing three molecules of the complex, $12WO_3.SiO_2$.

The first two may be, respectively:

$2(Al_2O_3 3H_2O) + 3(12WO_3 SiO_2 4H_2O) + 75H_2O$
$2(Al_2O.3H_2O) + 3(12WO_3 SiO_2.3(NH_4)_2O.H_2O + 63H_2O$

and the third:

$2(N_2O_3 Na_2O) + 3(12WO_3 SiO_2 2Na_2O 2H_2O) + 39H_2O.$

The following also deserve notice in the same connection:

$12WO_3SiO_2.3K_2O.H_2O + 12WO_3SiO_2.2K_2O.2H_2O + 26H_2O.$
$12WO_3SiO_2.3CaO.H_2O + 12WO_3SiO_2.2CaO.2H_2O + 46H_2O.$

The simplest view which can be taken of all these compounds is that they contain the complex $12WO_3.SiO_2.4RO$ and that they are double and not quadruple salts or acids. And it seems to me that the weight of evidence is in favor of the view that the complex $WO_3SiO_2.4RO$ or $10WO_3SiO_2.4O$ is single or unitary and not a double salt of a tungstate and silicate.

Marignac did not attempt to explain the isomerism of silicotungstic and tungstosilicic acids, and the problem is one of much interest, since there are no allotropic modifications of tungsten or silicon.

To obtain a further insight into the constitution of the simpler complex acids we may, with advantage, also consider the cases of the phosphotungstates and phosphomolybdates. As normal we may take the acids $12WO_3.PO_4H_3$ or $12MoO_3.PO_4H_3$. These correspond to the twelve molecule silicotungstates or tungstosilicates of Marignac, and the reasoning applied to these cases will apply also to the phosphotungstates and phosphomolybdates. Only in these last the number of known compounds is much greater, acids in which the proportion of tungstic or molybdic oxide to phosphoric oxide is as 24:1, 22:1, 20:1, 18:1, 16:1, 5:1, being known in their corresponding salts. Besides these we have arsenic compounds in which the ratio of tungstic or molybdic oxide to arsenic oxide is as 6:1 and 7:1, as well as double salts in the ordinary acceptation of the term. Instances of these last are:

(1) $22MoO_3.P_2O_5.3K_2O + 22MoO_3.P_2O_5.2K_2O.H_2O + 21\ H_2O.$
(2) $2(20MoO_3.P_2O_5.3(NH_4)_2O) + 20MoO_3.P_2O_5.2(NH_4)_2O.H_2O$
$+ 11\ H_2O.$
(3) $24MoO_3P_2O_5.3(NH_4)_2O + (24MoO_3.P_2O_5.2(NH_4)_2O.H_2O$
$+ 16\ H_2O..$

Upon any other than the view which I have taken the phosphotungstates and phosphomolybdates of the types

$$nWO_3P_2O_56RO \text{ and } nMoO_3.P_2O_56RO$$

are salts *sui generis* analogous to metatungstates and metamolybdates. The compounds expressed by these last formulas are quaternary.

We may compare these salts (1), (2) and (3) with the double tungstate

$$12\ WO_3.5Na_2O + 10\ WO_3.4Na_2O + 51\ aq.$$

They behave like salts of metatungstates or metamolybdates which have taken into their structure molecules of phosphoric or arsenic oxides without materially changing their general character. The precise mode of combination of the tungstic and molybdic molecular masses with silica, phosphoric or arsenic oxides is not at present known, but it is not difficult to show that the units of affinity of all the elements present may be accounted for and illustrated by structural diagrams. In like manner, the condensation of n molecules of tungstic or molybdic teroxide is easily explained, the number of unsaturated or residual units of affinity being for all values of n equal to 2, as in the case of water.[1]

The study of the complex acids which contain orthophosphoric acid as such naturally led to that of complex acids containing other acids of phosphorus. The greater number, or at least the more important, of these have been shown to give well-defined complex acids, and in all cases appeared to contain the phosphorus compound in the modification in which it existed before the combination with tungstic or molybdic oxide. Since this work was completed several new acids containing phosphorus have been discovered, more especially the so-called hypophosphoric acid, and it is to be hoped that these also will be studied from the same point of view. I shall ask attention to the compounds now known. The only pyrophosphotungstates yet observed belong to the general types for the acids

$$(22WO_3.9P_2O_7H_4)2RO$$

and for the salts

$$(22WO_3.9P_2O_7R_2)\ 2RO.$$

The salts containing molybdic teroxide differ from the above and belong to different types. All those which were examined contained manganous oxide and all contained but two molecules of pyrophosphoric acid or corresponding pyrophosphates in place of nine. It is not easy to see how the presence of manganous oxide as base can affect the formation of such salts, but the analyses of

[1] See American Journal of Science and Arts, Vol. XLIV, November, 1867.

a pyrophosphotungstate containing six molecules of manganous oxide correspond very closely to a formula embraced under the type

$$14WO_3.6RO + P_2O_7R'_4$$

so that, so far as the empirical constitution is concerned, the salt may be regarded as double and as containing two salts belonging to well-known types. In the present state of our knowledge it seems more probable that the pyrophosphotungstates and pyrophosphomolybdates are double salts belonging to the second class of complex inorganic salts. The solutions of the alkaline salts give, with solutions of the heavy metals, precipitates which contain only pyrophosphoric acid and neither tungsten nor molybdenum. On the other hand, it is well worthy of notice that the group

$$9(P_2O_7R'_4)$$

is capable of existing independently of tungstic oxide. Wallroth has shown that we have salts of the type

$$(P_2O_7)_9R''_{10}Na_{16}.$$

Of the other modifications of phosphoric acid I shall notice only the monometaphosphates and the hexametaphosphates. In the case of the former the type of the tungsten compounds is:

$$(mWO_3.2PO_3R)pR'O.$$

As in the case of the pyrophosphotungstates we have here to distinguish an internal and an external basicity, the group

$$(mWO_3.2PO_3R)$$

standing in the relation of a complex oxide to the external group $pR'O$. For the monometa-phosphomolybdates the type is different:

$$(m'MoO_3.4PO_3R)p'R'O$$

and, of course, other types are possible. So far as we can now judge the salts containing mono-metaphosphoric acid belong to the first group like phosphotungstates and phosphomolybdates of the type

$$12WO_3.PO_4R_3 \text{ and } 12MoO_3PO_4R_3.$$

They give no reactions characteristic of their constituents, but the precipitates which they form with metallic salts have the oily or pasty character of the salts of metaphosphoric acid.

The only hexameta-phosphomolybdate examined has the formula

$$(14MoO_3.6PO_3Ba_3)\,BaO + 55\ H_2O$$

and we may safely infer that the type is

$$(mMoO_3.6PO_3R_3)pRO,$$

there being an internal and external basicity, as in the cases above noted.

It is worthy of note that in all the known acids containing phosphorus in some oxidized form, other than orthophosphoric acid, the number of molecules of the acid united to tungstic or molybdic oxide is greater than in the orthophosphotungstates or orthophosphomolybdates containing an equal number of molecules of tungstic or molybdic oxides.

Two classes of phosphorus compounds remain to be considered —those which contain phosphorous and those which contain hypophosphorous acid or corresponding salts. Chemists are not wholly agreed as to the rational constitution of either of these acids, but it is perhaps more probable that the phosphorous molecule is represented by the formula

$$\{H.PO(OH)_2\{$$

and the hypophosphorous molecule by the formula

$$\{H_2PO.OH\}.$$

The typical formula for salts containing the former is

$$mWO_3.n\{H.PO(OR)_2pR'O$$

or

$$m'MoO_3.n'\{H.PO(OR)_2p'R'O.$$

and for those which contain the hypo-phosphorous molecule

$$mWO_3n\{H_2PO.OR\}pRO$$
$$m'MoO_3n'\{H_2PO.OR\}p'R'O$$

Both groups of salts appear to belong to the first class of complex acids or salts. It is worthy of notice that the phosphorous and hypo-phosphorous salts do not reduce copper from its solutions even on boiling, and they can, therefore, hardly be supposed to be simply double salts in the ordinary acceptation of the term.

We owe to Blomstrand the discovery of a specially remarkable and interesting class of complex acids, the molybdoperiodates or,

in the notation which is more commonly used, periodomolybdates. The acid of this series has the formula:

$$12MoO_3.I_2O_7.5H_2O$$

and is the analogue of phosphomolybdic acid,

$$24MoO_3.P_2O_5.3H_2O$$

The salts of this series are 5-basic, but, as in the case of metatungstates and metamolybdates, these are composed of two or more normal salts. The best defined of these has the formula:

$$(12MoO_3.I_2O_7.5K_2O) + (12MoO_3.I_2O_7.2K_2O.3H_2O) + 30\,H_2O.$$

which we may compare with

$$(24MoO_3.P_2O_5 3(NH_4)_2O) + (24MoO_3.P_2O_5\ 2(NH_4)_2O.H_2O) + 16\,H_2O.$$

In the complex acids, of which the present paper treats, we may consider the teroxides of tungsten and molybdenum as the determinants. Oxides, of which SiO_2, P_2O, R_2O_3 and I_2O_7 may serve as types, may be regarded as sub-determinants. The chemical potential of the compound will, of course, be a function of the chemical potentials of both determinants and sub-determinants. Protoxides appear not to form sub-determinants, but in all cases to act simply as bases. One of the difficulties of our subject consists in determining how far the sub-determinants proper may act as bases, and especially whether they are not sometimes partly bases and partly sub-determinants in the same salt.

In my published paper I have made two assumptions as regards the rôle of vanadic pentoxide in the vanadiotungstates and vanadiomolybdates.

The first is that $V_2O_2O_3$ may replace WO_3 so that chemically

$$mWO_3 + n\,(V_2O_2)O_3$$

may act as a whole; the second that V_2O_5 may form combinations with a number of molecules of tungstic or molybdic teroxide analogous to phosphotungstates or phosphomolybdates. These assumptions, taken separately or together as the case may require, in many cases at least reduce complex to simple formulas perfectly

comparable to those of salts containing phosphoric or arsenic pentoxide. Thus we have

$$5WO_3.V_2O_5.4(NH_4)_2O.2H_2O + 11\,H_2O$$
$$5MoO_3.V_2O_5.3\,(NH_4)_2O.3H_2O + 4H_2O$$

and

$$8MoO_3.\,V_2O_5.3BaO + 8MoO_3.2BaO.H_2O.$$

In addition I have described two vanadiotungstic acids having respectively the formulas:

$$10WO_3.V_2O_5.6H_2O + 16\,H_2O.$$
$$18WO_3.V_2O_5.6H_2O + 30\,H_2O.$$

The existence of these compounds has been denied,[1] but I see no sufficient reason for rejecting the formulas, which at least closely correspond to the analyses. The same remark applies to the acid

$$20V_2O_2.P_2O_5.6H_2O + 53\,H_2O$$

or

$$20V_2O_2O_3.P_2O_5.6H_2O + 53\,H_2O$$

which I would compare with

$$20WO_3.P_2O_5.6H_2O.$$

It must be admitted, however, that in view of the extraordinary number and variety of the combinations of vanadic pentoxide with bases it cannot be denied that there are very numerous compounds of this oxide with the teroxides of tungsten and molybdenum, which, in the present state of our knowledge, must be classed as double salts. The empirical composition of these salts may be expressed by the general formula:

$$mWO_3.nV_2O_5.pRO \qquad \text{or} \qquad m'MoO_3n'V_2O_5p'RO.$$

in which m, m′ represent respectively the number of molecules of tungsten or molybdenum and n, n′, p, p′ are whole numbers. Friedheim assumes that in many of these compounds vanadic pentoxide acts as a base and that in others mixtures of isomorphous salts are present. The pentoxide dissolves readily in the stronger acids, as, for instance, in sulphuric and phosphoric acids, but I am not aware that well defined crystalline salts of this oxide have

[1] See Friedheim. Berichte, Band XXIII, p. 1505 and 1530 (and Rosenheim Dissertation, Berlin, 1888); Zeitschrift für anorgan. Chemie, Band V, p. 437; Berichte, Band 24, p. 1173.

been obtained. There are also, I believe, no known instances of isomorphism between tungstic and vanadic or molybdic and vanadic oxides. In spite, therefore, of all that has been done, it seems necessary to admit that our knowledge of the vanadiotungstates and vanadiomolybdates is still very imperfect, but this remark applies also to the very large group of alkaline vanadates.[1] Of course, the theory that in the salts in question we are dealing, chiefly at least, with double salts is that which would naturally present itself first, but we meet with the difficulty that the constituents of these double salts are themselves double salts, so that we must deal in fact with triple and quadruple salts.

In my view the acids or salts which are usually termed complex may be divided into two classes.

Class first embraces those in which the union of the determinant and sub-determinant is so complete that the special characters of the constituents are nearly or wholly lost. The salts of this class are not decomposed by solution. They may be compared to certain so-called double cyanides, as, for example, to ferrocyanides and cobalticyanides. The acids $12\ WO_3.PO_4H_3$ and $12\ MoO_3.PO_4H_3$ are examples.

Class second embraces double salts proper which are decomposed by solution alone and which contain two different constituents. Salts of this class are very numerous, and there are probably various sub-classes.

In the present state of our knowledge it is not possible for us to assign to more than a few acids the classes to which they belong, and a vast field of chemico-physical work is open to the investigation of this subject alone.

We have also to determine by methods now well known to chemists the molecular masses of metatungstates and metamolybdates and of the complex inorganic acids, which may be considered as derived from, or at least as definitely related to them. Certain salts appear to be composed of metatungstates or metamolybdates united to salts containing sub-determinants, as for instance and more especially platinic oxide. I may, for the sake of clearness, refer to such formulas as

$$10\ WO_3.4Na_2O + 10\ WO_3\ PtO_2.5Na_2O + 58H_2O$$

and

$$4\{12MoO_3.2K_2O.3H_2O\} + \{12MoO_3.PtO_2.2K_2O.4H_2O\} + 24H_2O.$$

[1] See Proceedings of Am. Acad., Vol. XVIII, p. 74.

These salts appear not to be decomposed by water and to belong therefore to class first.

The compounds of arsenic oxide with tungstic and molybdic oxide closely resemble those of phosphoric oxide and do not now require special notice as regards their structure and properties, nor as to the question of their position as unitary or double salts.

On the other hand, the salts which contain arsenious oxide present some special points of interest, since this oxide exhibits distinct basic and, in its salts, distinct acid properties. The general formulas for this class of compounds, so far as they have been studied, are

$$mWO_3.nAs_2O_3.pRO \quad \text{and} \quad m'MoO_3.n'As_2O_3.p'RO.$$

As characteristic salts we may take the arsenosomolybdates

$$6MoO_3.3As_2O_3.2RO$$

of which four have been obtained in fine crystals.

The copper and nickel salts of this type have a fine green color and, therefore, contain the two metals as ions, at least in part. It is, therefore, most probable that the salts are unitary. This view is supported by the fact that the alkaline salts from which the salts of manganese, copper, zinc and nickel are obtained by double decomposition give reactions which are different from those of the constituent molybdic and arsenious oxides. The same statement applies to other known arsenosomolybdates and to arsenosotungstates.

I pass over without notice a number of interesting salts, some of which contain for a single determinant at least three different sub-determinants, as, for example, phosphoric and vanadic pentoxides and vanadic dioxide. We are only at the beginning of our knowledge of these compounds. From their extreme complexity it would seem most probable that they belong to the second class and are in some cases double, triple or quadruple salts, of which possibly the components may belong to the first class. Long and laborious investigations are required, sure to be rewarded by an abundant harvest of new facts, of little or no practical value perhaps, but to be looked at from the higher point of view of pure abstract knowledge.

The term "complex inorganic" was at first intended to embrace all the compounds containing a relatively large number of molecules of tungstic and molybdic oxides as determinants, beginning

with the silicotungstates of Marignac. Hittorf, in 1859, appears to have first drawn the distinction between double salts which are decomposed by solution and those which are stable under the same circumstances. Finally, Ostwald proposed to restrict the term "complex" to the salts which are not decomposed by solution and which give none of the chemical reactions of the constituents.

Of these two classes it is difficult to say which is the more interesting and theoretically important. The salts of the first class are comparatively new to chemists, and, in spite of all which has been done, offer a very wide field for investigation. Those of the second class present new species of double and even triple and quadruple salts. It will first be necessary to study the whole subject by physical as well as by the purely chemical methods which all chemists have hitherto employed, so as to define as distinctly as possible the limit, if there be one, which separates the two classes from each other. I make the point that all known methods of distinguishing double from unitary salts should be employed in each case, as I believe it has not yet been satisfactorily shown that all these methods give coincident results. It seems also possible that there may exist between the two classes given a third class, the salts belonging to which exhibit intermediate properties. In chemistry, as in other branches of science, sharply defined boundaries between groups are the exception. I am not aware that the chief methods of distinguishing between single and double salts have been brought together for reference, and perhaps the following list may be convenient:

1. Purely chemical or reaction tests.
2. Decomposition by solution alone.
3. Physical methods.
   a. Determination of the electric conducting power of dilute solutions.
   b. Determination of the ions by electrolysis and by spectroscopic observations.
   c. Observation of the change of temperature in the formation of the complex salt. Heat is evolved in the formation of a complex salt from unitary molecules; none in the formation of a double salt. The formation of a complex salt, therefore, is accompanied by a loss of energy.

Other methods have been given, but the above mentioned will suffice for most purposes.

In the brief and elementary sketch which I have given to-day of two large and important classes of salts I have avoided the use

of the so-called structural, or, as they should be termed, valence formulas. That such formulas are often useful, if only as checks upon purely speculative work, cannot be doubted; but it seems to me that the time has come for wider and more general conceptions of the chemical action between masses, subject perhaps to the fundamental idea of valence and yet not too dependent upon this idea, as at present received. In a paper published long since,[1] an attempt was made to extend the atomistic conception and to maintain the theory that the action of one complex mass upon another is determined by the residual unsaturated units of affinity present, without reference to the number of units due to each atomic mass. I venture now to suggest that, in addition to its valence, each atom and each molecule possesses a special chemical potential, not necessarily a function of its valence. The expression "chemical potential" is not wholly new, but I think the conception has never been clearly defined. I would now define it as bearing the same relation to chemical action which the electric potential bears to electrical action, the two potentials being mutually convertible, all chemical compounds having residual affinities or potentials besides the valences. If we suppose that the atoms within the molecule are in motion, such motion will be independent of the valences and the molecule will have a certain amount of free kinetic energy convertible into chemical or electrical energy or into heat.

In inorganic chemistry four great problems now present themselves for solution. These are:

The existence and chemical relations of the gaseous elements, of which five are known to exist in the atmosphere; the separation of the elements forming the rare earths, by systematic processes and the determination of their positions in the periodic series; the thorough, and so far as possible, exhaustive study of the complex and double salts; and, finally, the determination of the atomic masses of the elements with all the precision of which the subject admits, and in the spirit of Stas, of Richards and of Morley.

[1] *Loc. cit.*, p. 8.

SECTION A.

MATHEMATICS AND ASTRONOMY.

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ADDRESS

BY

EDWARD E. BARNARD,

VICE PRESIDENT, AND CHAIRMAN OF SECTION A.

THE DEVELOPMENT OF PHOTOGRAPHY IN ASTRONOMY.

THE American Association for the Advancement of Science has now completed an existence of half a century. It has become one of the leading scientific institutions of this country. Since its organization fifty years ago, the world has advanced with wonderful rapidity in all directions, and especially in the various fields of science. It is hardly too much to say that the scientific progress in the last half century far exceeds all that was done in the preceding thousand years. The life of this Association practically covers the development and comparative perfection of many of the sciences. This is especially true of the wonderful art of photography. At the beginning of the work of this Association, the great discovery of making pictures by the natural light of the sun had just been made, and while it aroused a widespread interest all over the world at that time, there were very few who dreamed of the great future value of photography in the arts and sciences. One of those who saw something of the future of Daguerre's discovery was the celebrated Scottish astronomer Dr. Dick, whose works on popular astronomy are still useful and delightful reading. In his Practical Astronomer, published in 1845, he said:

"It is not improbable, likewise, that this art (still in its infancy), when it approximates to perfection, may enable us to take representations of the sublime objects of the heavens. The sun affords sufficient light for this purpose; and there appears no insurmountable obstacle, in taking, in this way, a highly magnified

picture of that luminary which shall be capable of being again magnified by a powerful microscope. It is by no means improbable, from experiments that have hitherto been made, that one may obtain an accurate delineation of the lunar world from the moon herself. The plated discs prepared by Daguerre receive impressions from the action of the lunar rays to such an extent as permits the hope that photographic charts of the moon may soon be obtained and, if so, they will excel in accuracy all the delineations of this orb that have hitherto been obtained; and, if they should bear a miscroscopic power, objects may be perceived on the lunar surface which have hitherto been invisible. Nor is it impossible that the planets Venus, Mars, Jupiter and Saturn may be delineated in this way, and objects discovered which cannot be descried by means of the telescope. It might, perhaps, be considered as beyond the bounds of probability to expect that very distant nebulæ might thus be fixed, and a delineation of these objects produced which shall be capable of being magnified by microscopes; but we ought to consider that the art is yet in its infancy, that plates of a more delicate nature than those hitherto used may yet be prepared and other properties of light may yet be discovered which shall facilitate such designs."

Had Dr. Dick lived until the present day he would be amazed to see what portions of his prediction have in a measure come true. To him the most improbable of the things he forecast for photography to accomplish was the delineation of the nebulæ, and yet it is in this direction that photographic astronomy has most decidedly excelled. To use highly magnified images for photographing the details of the planets seemed to him to be among the first triumphs that were to fall to astronomical photography; yet today they are almost as far from realization as they were in the days when good Dr. Dick charmed his readers with vivid descriptions of the wonders of Astronomy. I do not think the most active imagination could have foreseen in Dr. Dick's day the marvellous extent to which astronomy at the close of the nineteenth century would be influenced by that light picturing process just then being developed by Daguerre and others.

After all, however, it is easy in the case of great discoveries of this kind, to predict what they will amount to. This is usually done by immensely exaggerating all their possibilities and thus, by a happy chance, hitting one or more of their realities; for strict

account is usually kept only of the hits in such cases, the misses being rejected by a charitable world as a matter of no importance. We are used to wonders in these days of wonders, and have a happy habit — from frequent practice — of correctly guessing the outcome of some of the great discoveries. But when Dr. Dick wrote, these things were not so easily foreseen, for the possibilities of the sciences were not so apparent then as they are now.

So great have become the possibilities of photography in the astronomical investigations of to-day, that an account, in detail, of its accomplishments would far exceed the limits of this paper, and for that reason I shall be forced to a brevity in dealing with this subject that must necessarily pass over many of the interesting things photography has done for astronomy in its comparatively short lifetime.

It does not come within the province of a paper of this kind to deal with the question of priority in the discovery of photography (though something might be said on that point for America), as the process interests us only so far as its application to astronomy is concerned.

It appears that on the very first announcement of Daguerre's wonderful discovery on the 19th of August, 1839, the celebrated French astronomer, Arago, who addressed the Paris Academy on the subject, quickly foresaw the great advantage it must necessarily be to the science of Astronomy, especially in the faithful delineation of the surface features of the sun and the moon, for these two objects, at least, were bright enough to register themselves with the sluggish materials then in use. It is especially gratifying to Americans that the first efforts to utilize the new discovery for the benefit of astronomy, were made in this country; and that Americans have always been promptly identified with the process from its very earliest conception.

Within less than one year from the announcement of Daguerre's discovery, in March of 1840, Dr. John W. Draper, of New York City, had succeeded in getting pictures of the moon which, though not very good, foreshadowed the possibilities of lunar photography. Five years later the Harvard College Observatory may be said to have commenced its remarkable career of astronomical photography, when Bond, with the aid of Messrs. Whipple and Black, of Boston, succeeded in getting still better pictures of the moon with the 15-inch refractor. These pictures, on daguerreotype

plates, seem to have been fairly good, and to have shown much detail, though the telescope was wholly uncorrected for the photographic rays. They attracted a great interest in the subject, especially in England, but the difficulties encountered led to failures generally, except in the case of De La Rue, Dancer, and one or two others. To Dancer is doubtless due the earliest success in lunar photography. Excellent photographs, it is said, were made by him as early as February, 1850. In 1858, De La Rue, using a 13-inch speculum, without clock-work, and guiding by following a lunar crater seen through the plate, made the most important of the early efforts at lunar photography. From this time De La Rue made the best pictures of the moon until the subject was taken up again in America in 1860 by Dr. Henry Draper, son of the illustrious John W. Draper. Like De La Rue, Dr. Draper constructed his own telescope, a 15½-inch reflector. With this instrument he secured excellent photographs of the moon, superior to any previously made, and capable of considerable enlargement. These pictures of the moon were the best taken until Lewis M. Rutherfurd began his remarkable work about 1865. Rutherfurd's work marked the most important step until then made in astronomical photography. From this time on he produced such admirable photographs of the moon that they have not been excelled until within the past few years. These were made with a refractor of eleven inches aperture, which had been constructed under his immediate supervision. This was the first telescope corrected specially for the photographic rays. Some excellent lunar photographs, in the meantime, had been made with the great 4-foot reflector of the Melbourne Observatory.

The completion of the Lick Observatory in 1888 marked another decided advance in the photography of the moon. The great focal length of this magnificent instrument gave an unenlarged image of the moon about six inches in diameter, which in itself was a great advantage.

The admirable lunar photographs made by MM. Loewy and Puiseux, with the Equatorial Coudé, at Paris, in the past few years, have excelled anything yet made in this direction.

But what is shown by the best lunar photographs has not yet approached that which can be seen with a good telescope of very moderate size. The minute details are at present beyond the reach of photography, but its accurate location of the less difficult features

is of the highest value. The greatest interest in any observation of the moon would be in any changes that might take place on its surface. It has long ago been shown that no changes on a large scale have occurred in recent times. It is therefore to the minuter details, if anywhere, that we must look for change in the moon, and these at present are much beyond the reach of the photographic plate.

The sun, from its abundance of light, offered special inducements to the early workers in photography. It would rather appear, however, that the moon had the most charm for the first photographic astronomers. This was doubtless due to the singular and wonderful wealth of detail its surface continually presented; while the sun, except for a few occasional spots, was at best only a blank surface. But when carefully and conscientiously studied, however, it highly rewarded those who took up its study photographically.

The first picture of the sun seems to have been made on a daguerreotype plate by Fizeau and Foucault in 1845. During the total eclipse of the sun on July 21, 1851, a daguerreotype was secured with the Königsberg heliometer (two and four-tenths inches in diameter and four feet focus) by Dr. Busch, which appears to have been the first photographic representation of the corona. It showed considerable detail quite close to the moon. But in the early eclipses the photographic work seems to have been mainly devoted to representations of the solar prominences, which at that time were as rarely seen as the corona itself. During the eclipse of 1869, however, Professor Himes secured a photograph which showed the brighter structure of the corona; similar pictures were also obtained during the same eclipse by Mr. Whipple of Boston. The corona was also slightly shown on pictures made as early as 1860 by M. Serrat. None of them, however, showed more than slight traces of the corona, extending only for a few minutes of arc from the moon's limb. Nearly all the pictures seem to have been taken with an enlarging lens, which was doubtless used to get the prominences on a larger scale. Mr. Whipple, however, in 1869, did not use any primary enlargement, and this gave him a decided advantage in point of exposure time. In nearly all of these pictures, the exposures with the slowness of the then existing plates, were evidently too short to show the corona, except perhaps in the case of Whipple, who gave forty seconds exposure.

The first really successful photographs of the corona were ob-

tained at the eclipse of Dec. 22, 1870, when the corona was shown on the plate to a distance of about half a degree from the moon's limb. This picture, made by Mr. Brothers at Syracuse, Sicily, showed a considerable amount of rich detail in the coronal structure; and the same can also be said of the photographs of this eclipse taken by Colonel Tennant and Lord Lindsay's party. These seem to have been the first pictures to show really the great value of photography for coronal delineation. The eclipse of 1871 was still more successfully photographed, and an excellent representation of the corona, full of beautiful detail, was secured.

All of these pictures were made with the wet process, for the dry plate was not successfully used until about 1876, and it was five or six years later, before it became generally useful or at all reliable. For some years previous to this, photographers had been at work with varied success upon different methods of preparing and sensitizing plates that could be used dry. Indeed, at the eclipse of 1871 at Baikul, India, it appears that Mr. Cherry used a dry plate on which he exposed a number of images of the sun before totality for the orientation of the eclipse plates. It would seem that he had only two of these plates and they were both intended to be used for orientation. The second plate, however, was broken before it could be used. It is probable they were known to be less sensitive than the wet plates, which doubtless prevented their being used for the corona.

Though the pictures of 1870 and 1871 showed the value of the photographic method, it had so far failed to show the greater, and fainter extensions of the corona. In speaking of the eclipse of 1871 and the success attained by photography at that time, it is well here to mention the evil influence the old method of drawing had upon, at least, one observer. A Mr. Holiday had made observations of the corona from a housetop, and thus describes the result in his report:

"As soon as the eclipse was over, I came down from the roof and plunged my head into cold water, for I was violently excited, and before breakfast I had made three drawings from my memoranda." If photography has done nothing more for astronomy than to prevent occurrences of this kind it would at least deserve lasting respect from a humane point of view.

In 1878, extensive preparations were made to observe the eclipse of July 29 of that year. Photography was to play an important

part, though astronomers did not rely very strongly upon it, for it appears that all were prepared to make the customary drawings of the corona, and unfortunately each person faithfully carried out that purpose. A most suggestive illustration of the uncertainty of such work is found in the large collection of drawings published in a volume issued by the United States government relating to the eclipse of 1878. An examination of these forty or fifty pictures shows that scarcely any two of them would be supposed to represent the same object; and none of them at all closely resembled the photographs. The method of free-hand drawings of the corona, made under the attending conditions of a total eclipse, received its death blow at that time, for it showed the utter inability of the average astronomer to sketch or draw under such circumstances what he really saw.

At this time the dry plate was still in its infancy, and the results with it ranged from failure up to a fair picture of the corona. The greatest extension of the corona obtained, however, seemed to be about half a degree from the moon's limb, while Professors Newcomb and Langley traced it nearly six degrees with the eye alone. The results, nevertheless, were highly important, and they demonstrated the success of photography for this class of work. One of the cameras at this eclipse was put in charge of a private soldier, with instructions to give an exposure of sixty-five seconds. The instructions were faithfully carried out, but as no one had told him to draw the dark slide the plate remained unexposed.

The total eclipse of 1882 proved to be of special interest, from the fact that a small unknown comet was then in the immediate neighborhood of the sun, and was seen with the naked eye during the eclipse. The increased sensitiveness of the plates then in use secured a strong impression of this object. The known history of the comet is comprised within the few minutes of totality of that eclipse, for it was never seen afterwards.

In the eclipse of 1886 photography again held an important position. But the extremely humid climate of Granada (one of the observing stations) and the necessity of employing volunteer observers, led to numerous disasters, such as the failure to get the sun's image on the sensitive plate in the most important instrument, the breaking of the polar-axis just before totality in the next important instrument, the failure of an assistant to make the exposures with another until totality was all but over, the fact that two native policemen stood in front of the photometer during totality,

the two weeks' delay of the steamer in getting away from the island, the seizure of the plates by the customs officials on arrival at New York, and, after their rescue, the subsequent delay for want of time to develop the plates, until May of the next year when they had undergone decomposition, so that the results were not as good as might have been expected.

The closing of the year 1888 and the opening of 1889 brought one of the most important eclipses that had yet occurred from a photographic standpoint. Certainly no previous eclipse nor any since, so far as that is concerned, was photographed by so many different persons, and with such a varied assortment of cameras, telescopes, etc. The path of this eclipse lay across Nevada and California, and every photographer, amateur or professional, near the line of totality, took part in the work. The amateur photographers of San Francisco and Oakland banded together under the leadership of Mr. Charles Burckhalter and photographed the eclipse in a systematic manner, the result being a most excellent collection of negatives of the corona. In some of these pictures the coronal streamers were carried to a far greater extent than at any previous eclipse; especially was this so, in the photographs made by two of the amateur photographers, Messrs. Lowden and Ireland. At this eclipse the lot fell to the writer to make the photographs for the Lick Observatory. But at this time the observatory had no instruments suitable for the work. To secure as large an image as possible with the poor equipment at hand, a three and a half inch visual objective, by Alvan Clark, was selected. This lens after being reduced to one and three-fourths of an inch in diameter and mounted in an oblong box, fastened to a polar axis driven by the clock-work belonging to the 12-inch equatorial, was found to give a fairly good photographic image. With this and two small photographic cameras, nine negatives of the corona were secured. The best of these was one made with the Clark visual objective. By extreme care in development, this negative not only showed the exquisite polar systems of streamers and the details of the corona close to the moon, but also carried the coronal extensions a great distance along the ecliptic. This was by far the most successful eclipse photographically of any that had yet been observed, and forever set aside as worthless the crude and wholly unreliable free-hand sketches and drawings previously depended upon.

The eclipse of December 22, 1889, was successfully photo-

graphed, among others, by Mr. Burnham and Professor Schaeberle, comprising the Lick Observatory eclipse expedition which was sent to Cayenne. It was at this eclipse that Father Perry lost his life through the trying climatic conditions. With the sickness of death upon him, this brave man, fearless in his duty, stood by his cameras and carefully carried out his program during the eclipse, only to collapse at its close, and die a few days later on the vessel that was carrying him away from the fateful spot.

The eclipse of 1893 was successfully photographed in Brazil, Africa and Chile. Professor Schaeberle made arrangements for the photography of the corona on a large scale, and with his apparatus at Mina Bronces, Chile, secured a fine series of photographs with a photo-heliograph of forty-feet focus, which he mounted on a hill sloping towards the sun. The image was formed by a stationary lens five inches in diameter upon a large sensitive plate which was moved by clock-work to counteract the sun's motion during the few minutes of the eclipse. In these pictures the image of the sun was on such a large scale that the coronal details could be very accurately studied. Upon these plates, Professor Schaeberle found a hazy ill-defined spot at forty minutes' distance from the sun's center. This he subsequently found also on the African and Brazilian plates, taken by the English and American astronomers. This object was in motion away from the sun, as shown by the photographs which covered an interval of $2^h\ 36^m$ of absolute time. Professor Schaeberle believes this was a comet. It is not impossible, however, that it was a mass of coronal matter moving out from the sun, such as has been shown by the spectroscope frequently to occur in the case of solar prominences. The fact that the object seemed to be connected with the sun by a coronal streamer would rather favor the explanation. The drawing given in Astronomy and Astro-Physics, p. 307, Vol. XIII, seems to support further this idea.

During the solar eclipse of 1896 the sky was cloudy at nearly all the stations, and especially where the most elaborate preparations had been made for photographic work. A few photographs were obtained, however, some of which, with very small lenses, showed the coronal extensions to a great length. The most important photographic work at this eclipse was the photographing of the flash spectrum, or the momentary reversal of the Fraunhofer lines which occurs when the edge of the sun disappears behind

the moon or reappears from it, and for an instant exposes the reversing layer, which was first seen by Professor Young at the eclipse of 1870. This extremely important picture was made by William Shackleton, a young Englishman, who patiently waited and watched the spectrum at the edge of the sun, and at the instant of the reversal of the lines exposed a plate which caught for the first time the fugitive bright lines which are only visible for about a second. This photograph was a triumph for photography, for the record of the phenomenon now does not rest upon the authority of any hasty observations, but remains a permanently visible record.

Our record closes with the eclipse of January 22, 1898, which was photographed in India by American and English astronomers. The photograph of the flash spectrum was successfully repeated by many observers. The coronal extensions at this eclipse were carried to a greater extent on the photographs than at any previous eclipse, by photographs secured with a very small lens by Mr. and Mrs. Maunder. The observations at all the stations were eminently satisfactory.

In speaking of the photography of the corona it is well to mention the extremely interesting experiments of Dr. Huggins in an endeavor some years ago to photograph it without an eclipse. By the use of absorbing media and later with extremely short exposures, he obtained very corona-like appearances, and it is not yet certain that they were not true coronal forms. Such experiments should be tried at very high altitudes in a pure atmosphere, and it is to be hoped that these efforts will be again undertaken under more favorable conditions.

The transit of Venus across the sun's disc in 1882 was very successfully photographed. The measures of the pictures taken by the Americans have given a good redetermination of the solar parallax. A fine series of photographs of this transit, with wet plates, was obtained by Prof. D. P. Todd at the Lick Observatory. This was six years before the completion of the observatory.

There is no question but Janssen, of Meudon, succeeded many years ago in making the best photographs of portions of the sun's surface that have yet been made. These pictures show the granulation of the solar surface and the details of the sun spots with admirable clearness. This astronomer has always used the old wet plate process, which seems to give the best results in solar

work. The instrument with which his work is done is a very crude affair. The lens, five inches in diameter, is placed in a wooden box, which is mounted on an old camera stand on rollers. It is not provided with clock-work. When Janssen wishes to make a photograph of the sun, he wheels this primitive affair, stained and daubed with nitrate of silver, from a shed to a platform, elevates it towards the sun, makes an exposure with a rapidly-moving slit, and secures a photograph, which, so far, but few have approached in excellence. These pictures are enlarged by a secondary lens in the camera box to about twenty inches in diameter. One peculiar feature of these photographs is the frequent presence of blurred regions, in striking contrast to the generally exquisite sharpness of the granular surface. These disturbed regions are believed by Janssen to be due to actual disturbances on the sun's surface, and therefore to be true phenomena of the sun. I have always had the impression that they are simply due to the presence of small areas of bad "seeing" which are passing at the moment of exposure; that is, they are the effects of small local disturbances in our own air such as every visual observer is familiar with in night work. I remember having once secured a photograph of the moon with the 12-inch of the Lick Observatory which showed just such a blurred spot on its surface. This question could be easily settled with a few exposures made a minute or so apart, for if the blurred appearance repeats itself at the same point on the sun's disc, then it cannot be due to local atmospheric disturbances. Doubtless M. Janssen has long ago decided this question, but, if so, it has escaped my notice.

For the first successful photographs of the sun's surface, however, we must return to America. The first pictures to show this granulation and the details of the sun spots were taken by Lewis M. Rutherfurd in 1870. These pictures were also made with the collodion, or wet process.

From the importance of a more thorough understanding of the effects of the sun upon the climate of the earth, daily photographs of the solar surface are made at a number of observatories, principally at Greenwich, Kew and in India. These have been kept up for a great many years. The Lick Observatory has in recent years also taken up this subject. It is scarcely probable that a single day goes by without photographs of the sun being made at some one of these observatories. Thus a valuable record is kept of the changes taking place on the solar surface. Just what effect these

sun storms have upon the earth is not yet definitely understood, but there seems to be an almost certain connection between some of the solar disturbances and terrestrial magnetic storms, so that when the sun is violently agitated a corresponding disturbance of the earth's magnetism occurs. It is not yet seen just how these disturbances may affect the weather; so far the testimony seems inconclusive, and local conditions on the earth may fully compensate for any effect solar storms might have here. In the meantime the work done in this connection at Greenwich and other places will continue to grow in importance. One thing that this repeated and constant photographing of the sun has proved is the non-existence of the so-called intra-mercurial planets, which before the days of photography were so frequently seen transiting the sun by Lescarbault and many others. No strange thing, aside from an alleged comet, which was afterwards traced to a stain on the photographic film, has been shown on any of these photographs, with the exception, perhaps, of one of the sun photographs made in India which caught a distant bird in its flight and showed clearly its head and outspread wings projected against the sun.

Just as this continuous photographing of the sun's surface has forever disposed of the alleged frequent transits of intra-mercurial planets, so will the photographic plate finally, when it has attained more perfection in dealing with the planets, snow that many of the strange features ascribed to the surfaces of some of them do not exist.

As I have said, the early photographs of eclipses seem to have been made with the sole end in view of securing pictures of the solar prominences. This was very important at first, for by the photographs it was proved that they were true solar phenomena. Possibly also in the very first photographs a picture of the corona was considered a hopeless matter on account of the lack of sensitiveness of the plates. The eclipse of 1868 is memorable for having shown Janssen and Lockyer that the visibility of the prominences did not necessarily depend upon a total eclipse of the sun. They found that by the aid of the spectroscope the prominences could be seen and studied at any time. This was an extremely important step, and placed our knowledge of the nature of the prominences upon a firm and lasting footing. The fact that these objects could be made visible with the spectroscope soon suggested to Professor Young the idea that they might also be

photographed at any time; and in 1870 he made efforts to secure impressions of them upon the photographic plate, and met with partial success. To photograph these objects successfully, however, required the invention of a special instrument, for the older methods must necessarily result in failure. In observing the prominences visually with the spectroscope, it is necessary to examine them through a slit which is very narrow compared with the height of the prominences; only a small section of the prominence can therefore be seen at once, and to see it all, the slit must be moved over the prominence. If the slit is widened, more of the object is shown, but at the same time such a flood of light is admitted that it is lost in the glare. If an instrument could be devised whereby the slit could be moved in front of a photographic plate, successively exposing to the plate all parts of the prominence, it will readily be seen that the entire image could be photographed. To do this there must be two slits moving in perfect unison—one placed across the sun in front of the grating or prism, the other in front of the photographic plate and adjusted perfectly to the spectral line of the prominence, so as to exclude all light save that emitted by the prominence itself; and thus, by the gradual motion of these two slits, the entire object is successively uncovered and an exact photograph secured of it.

The solar prominences consist mainly of incandescent hydrogen and calcium. The best results are secured by calcium alone. It is curious to see photographs of the same prominence made by using the hydrogen and calcium lines independently; these pictures often differ considerably, thereby showing the peculiarity of distribution of the calcium and hydrogen in the same prominence. The two components are differentiated, and it is thus shown just what part each component takes in the composition of the prominence. To make one of these pictures takes several minutes of exposure, during which time the slits slowly travel over the region of the prominence. This extremely ingenious device owes its existence to the inventive genius of Professor Hale, who devised and built the first instrument of this kind, and secured the first actual spectroscopic photograph of the prominences. These first pictures were made in 1891. It is therefore now a matter of no great labor to make not only photographs of single prominences, at any time, but through a further ingenious extension of the possibilities of the instrument it is made to move across the entire sun's disc,

thus securing every prominence at that time visible. By hiding the sun's image by an occulting disc in the first sweep, and then making a second similar but more rapid sweep with the sun's image uncovered, the sun itself with all its faculæ, spots, etc., is impressed in the blank space left for it, and a complete picture of the sun and all its surroundings, with the exception of the corona, is secured. This is the method employed by Professor Hale in his work. These pictures, however, show only those features of the sun which are due to hydrogen or calcium, and the solar surface thus appears very different from the telescopic view of it. The calcium regions come out with extraordinary distinctness; so much so, indeed, as at times to completely obliterate the sun spots, which at that moment are so distinct to the eye with the same telescope. Admirable work of this kind has also been done by M. Deslandres of the Paris Observatory, who has devised an instrument similar to that of Professor Hale.

From the first photograph of a star by Bond in 1850 to the present time, stellar photography has gradually risen to a prominence as remarkable as it is important. The real increase of importance in this work, however, has occurred within the past ten or fifteen years, since the successful introduction of the very rapid dry plate. The wet or collodion process was poorly adapted to the photography of the stars, and of no use whatever for comets and nebulæ. As implied by the term "wet process" the plate must remain wet through the entire work from its first coating with collodion until its final washing as a negative. The exposure time must therefore be very limited. Not only was the exposure of limited duration (from fifteen to twenty minutes), but the plate was very slow in its action compared with the dry plate of to-day. The combination of these two difficulties made it impossible to photograph anything except the brighter stars. Dr. Gould at Cordoba managed to increase the exposure time by keeping a stream of water playing over the plate. This however, might cause a deterioration of the film. With the inherent difficulties of the wet plate to contend with, it is little wonder that no faint stars, nebulæ or comets were photographed. Notwithstanding this, the photographs of the star clusters, etc., of the southern skies obtained under the direction of Gould with an 11-inch photographic refractor by the wet process, were of the highest value, and showed upon measurement a striking agreement in accuracy with visual work. The

same can be said of Mr. Rutherfurd's photographs of the Pleiades, Præsepe, etc., which were made prior to Dr. Gould's and which were the first photographs of this kind. These extremely valuable photographs of Rutherfurd are now receiving a most thorough measurement under the careful supervision of Prof. J. K. Rees of Columbia College, where Mr. Rutherfurd's negatives are stored. To this institution Mr. Rutherfurd left his telescope and measuring instruments.

In 1857, Bond had shown by measurement of a series of photographs of the double star Mizar that the highest confidence could be placed in measures of star plates. This has been fully verified in late years by Gill, Elkin and others. Dr. Elkin showed in 1889 that measures of a photograph of the Pleiades, taken by Mr. Burnham with the great telescope at Mt. Hamilton, had equal value with his heliometer measures of the same stars.

From the necessary conditions, the collodion process could make no advance in stellar photography. Previous to 1876 experiments had been made to get a workable dry plate and for the next six years more or less success had been attained in their manufacture. But the photographers themselves took hold of these plates with much caution because of their uncertainty. By 1881 or 1882, however, they were beginning to be used and gave considerable promise of their ultimate value, as was shown by the photographs of the comet of 1881, which were made by Draper and Janssen. These were the first photographs ever taken of a comet. Efforts had been made to secure pictures of Donati's comet, in 1858, but without success.

At the present time these plates are made so sensitive that a rifle bullet can be photographed in full flight; yet some of the celestial objects now photographed are so faint that it requires many hours' exposure with these same plates to show any trace of them.

In the fall of 1882 the world was thrilled by the advent of a magnificent comet which suddenly appeared near the sun in September, and for the next four or five months delighted astronomers with the splendor of its display. Attracted by the great brilliancy of the comet, Dr. Gill at the Cape of Good Hope, with the aid of a local photographer and his photographic lens, secured a fine series of photographs of the comet with dry plates. When these photographs reached the northern hemisphere they attracted a great deal of attention, not only on account of the comet itself,

but also from the number of stars that were impressed on the plates. At this period the Henry Brothers were making a chart of the stars along the ecliptic in their search for asteroids. They had at this time reached the region of the Milky Way, and the marvellous wealth of stars they encountered upon entering the boundaries of that vast zone, completely discouraged them in their endeavors to carry their charts through the rich region traversed by the ecliptic. While hesitating as to the advisability of continuing their work, the photographs of the great comet came to their notice. They were struck with the great number of stars shown on these pictures along with the image of the comet. The idea at once occurred to them that they could use this wonderful process to make their charts. It was to this simple incident that the active application of stellar photography of to-day is due. They began at once the construction, with their own hands, of a suitable photographic telescope of thirteen and one-half inches in diameter for the photography of the stars. This instrument was soon finished, and the astronomical world knows to-day what wonderful results these men produced with it: the exquisite star pictures which were marvels of definition, the photographs of the nebulæ, of Saturn and Jupiter, the moon, etc., were perfect revelations.

In 1859 Tempel, at Florence, Italy, had found a diffused cometary-looking nebula connected with and extending southwesterly from the star Merope of the Pleiades. From that time on astronomers wrangled over this object, which many of them believed had no existence. One of the first things done by the Henry Brothers was to photograph the Pleiades. These pictures showed nebulous strips near Merope, and though they did not resemble any that had been drawn by the numerous observers of the Merope nebula, they rather confirmed the existence of Tempel's object. Upon these plates was shown a new nebula connected with the star Maia where none was previously known. It required the most powerful of existing telescopes to verify this visually. This was finally done however, and it then began to dawn upon astronomers what great possibilities lay in the photographic plate for the detection and study of the nebulæ. It was soon seen that their light was strongly photographic; that it was really more actinic than visual. A later photograph with a longer exposure showed the Merope nebula just as the best observers had drawn it, and at the same time filled the entire group of stars with an entang-

ling system of nebulous matter which seemed to bind together the different stars of the group with misty wreaths and streams of filmy light, nearly all of which is entirely beyond the keenest vision and the most powerful telescope. This was a revelation: the question had often been asked whether it would ever be possible to photograph as faint a celestial body as could be seen with a powerful telescope. Here was the answer. It was not only possible to photograph some of the faintest objects seen in the telescope, but it was possible also to photograph some others which could never be seen in the sky. In one of his reports admiral Mouchez called attention to the fact that the only way they could see the satellite of Neptune at the Paris Observatory was on the photographs made by the Henry Brothers, for they had no telescope sufficiently powerful to show it visually.

The Henrys applied themselves assiduously to celestial photography, with the most remarkable success. They led the world in this work. While they were at the height of their activity, astronomers elsewhere were but beginning to awaken to the great importance of the subject. And yet there seems to have been essentially no public recognition of the work of these two men to whom astronomy owes so much. In personal appearance and temperament they are so extremely dissimilar that one would scarcely take them to be brothers. Up to the time they began their photographic work, they had between them discovered fourteen asteroids by the slow and tedious visual process of picking them out by their motion from the countless thousands of small stars. If one examines a list of the asteroids, he will be struck with the manner in which these fourteen small planets are recorded. According to this list the first one of these was discovered in 1872 by Prosper Henry, the next one by Paul Henry, and so on alternately throughout the entire fourteen, until 1879, when the last one found was attributed to Paul Henry. It is a curious fact, and one which will be readily understood by all who are acquainted with the unselfish affection existing between these two brothers, that the credit for the discovery of these fourteen minor planets is ascribed alternately to Prosper and Paul Henry. It is likely we shall never know which brother discovered any one of these planets.

Singularly enough, the photographic plate not only did away with the necessity of making these charts by eye and hand to facilitate the discovery of asteroids, but it also did away with the

necessity of the charts themselves for that purpose; for the little planet, which is moving among the stars, now registers its own discovery by leaving a short trail — its path during the exposure — on the photographic plate. The first of these photographic discoveries of asteroids was made by Dr. Max Wolf, in 1892. They are now found wholesale in this manner by photography.

It was the success of the Henry Brothers' work that led to the International Astro-Photographic Congress which met at Paris in 1886. It was their work that caused this Congress to meet at Paris. The Congress adopted the Henry Brothers' lens as a model for the instruments to be used, and the work of this great undertaking was based on that of the Henry Brothers. It was stated once by Admiral Mouchez that every object glass at the Paris Observatory had either been made by the Henrys or refigured by them.

Perhaps, as Dr. Dick himself thought in the early days of photography, the most unpromising subject for the photographic plate to deal with was the nebulæ. Most of these objects appeared so feeble in their light that but little encouragement in that direction was offered the celestial photographer.

One of the brightest and most promising of the nebulæ is that in the sword of Orion, and this was naturally one of the first of these objects to receive photographic attention. In September, 1880, Dr. Henry Draper began the photography of the nebulæ with this object, and succeeded with fifty-one minutes' exposure, using the dry plate, in getting a good picture of the brighter portions of the nebula. This was the first nebular photograph. With 104 minutes' exposure in March, 1881, with an 11-inch refractor, he secured a still better plate which showed stars down to the 14.7 magnitude, which were visually beyond the reach of the same telescope. But in March, 1882, he obtained the best picture of this wonderful nebula with an exposure of 137 minutes. These pictures marked a new era in the study of the nebulæ. When these results were communicated to the French Academy by Dr. Draper, Janssen took up the subject with a silver-on-glass mirror of very short focus, having the extraordinary ratio of aperture to focus of $\frac{1}{3}$; the aperture being twenty inches, with a focus of sixty-three inches. This remarkable instrument was constructed in 1870 for the total solar eclipse of 1871. With this Janssen found it easy to photograph the brightest parts of the nebula with comparatively

short exposures. This extremely powerful photographic instrument seems to have been unused for the past fifteen years; but very recently it has been brought into use again I understand with the most astonishing results in photographing the nebulæ. Unfortunately for science, the death of Dr. Draper in 1882 put a stop, in America, to the work he had inaugurated. But it was at once taken up in England by Common, who with a 3-foot reflector attained rapid and immediate success. His photographs of the great nebula of Orion are still classic. They were a great advance over the work of Draper for the reflector was not only a larger telescope, but was also better adapted for photographic purposes, and especially for photographing the nebulæ. In January, 1883, with only thirty-seven minutes' exposure, he secured what was by far the most striking and beautiful picture which had yet been taken of the great nebula. These pictures greatly extended the region of nebulosity and the delicate details were also better shown.

The writer remembers how much he was impressed a few years later with the beauty of one of Common's photographs. It created in him the first ambition to do work of this kind. Indeed, this picture, and one of a densely crowded region of a part of the constellation of Cygnus by the Henry Brothers, first called his attention to the great value of the photographic plate for astronomical purposes. It was at this time that the writer conceived the idea of photographing the Milky Way, though the experiments were not then successful for the want of a proper instrument. The great nebula, which has always had such a fascination for astronomers, was subsequently taken up by Isaac Roberts, who, by very prolonged exposures, still further extended the nebulous region and secured very beautiful pictures of it. Among the finest photographs of this object that have been made in recent years is one taken by Dr. H. C. Wilson at Northfield, Minn., with an 8-inch photographic refractor with an exposure of nine hours. The amount and sharpness of detail shown on this beautiful photograph are very striking, and essentially embrace all that has been done on this nebula by a photography up to the present time.

The great nebula of Andromeda has been known to astronomers for upwards of a thousand years, as it is a rather noticeable object with the naked eye. It has received its full share of attention ever since the invention of the telescope. It also appears to be a vast body of gaseous matter shining far away in the depths of space,

but it seems to be in a different condition from that of the great nebula of Orion, for its spectrum is continuous and does not show the bright lines, indicating an incandescent gas, which are present in the spectrum of the Orion nebula. Though the mystery of its physical condition has not yet been solved, photography has at least shown us its true form. It has ceased to be the long spindle-shaped body of the elder Herschel, or the broad irregular object, with two dark parallel channels in one side, as drawn by the Bonds and Trouvelot. The photographic plate reveals to us in their place a beautiful symmetrical mass of nebulous matter surrounded with several more or less concentric rings, claimed by some astronomers to be a representation of the nebular hypothesis of La Place in full operation. The first picture to show the true form of this wonderful object was taken in 1885 with a 20-inch reflector by Roberts. It does not, however, require a powerful photographic telescope to show its peculiar features, for a 6-inch portrait lens will show the rings well with anything above one hour's exposure.

Important photographs of some of the spiral nebulæ, and especially of the "whirlpool nebula" of Lord Rosse, were made as early as 1888 by von Gothard with a 10-inch reflector. Excellent photographs of these objects have also been obtained by Dr. Roberts.

While it is absolutely necessary to use a considerable photographic telescope for the accurate registration of star positions, etc., where measures of precision are required, there are a great number of objects in the sky which are not necessarily subject to measurement, and which for their greatest value require a simple pictorial representation. The Milky Way, one of the most beautiful and certainly the most stupendous of the celestial features, is not susceptible of accurate measurement. The individualizing and measurement of all its stars would be the most hopeless task imaginable. Nor would such a task be of any very great importance could it be accomplished as a whole, for we could not form any special idea of its structural peculiarities from such a work. Though a conspicuous object to the unaided eye, the view we thus get of it is not sufficiently tangible, from the lack of details, to enable one to form any more than a crude idea of its coarser features. But even the naked-eye view of it is far more comprehensive than a catalogue would be containing accurate determinations of all its individual stars. What is required therefore in the study of this wonderful object—this mighty universe of stars— is some-

thing that will increase the penetration of our vision, and at the same time give us a certain amount of accuracy of position with a large field of view, so that we may study its peculiarities of structure in detail, and at the same time closely locate these details with reference to the whole; and thus, by finally putting structure and detail together, form a comprehensive idea, not only of the details themselves but also of the relation of these features to each other. The long-focus telescope with a very limited field is not capable of dealing with the Milky Way in the manner stated. Its structural details are very large, far larger in general than is the field of view of the ordinary photographic telescope, and vastly greater than that of a powerful visual telescope. We want therefore a short-focus instrument, one capable not only of taking in a wide part of the sky, but also of giving a brilliant image, or in other words, the reduction of the large details to a smaller scale with a correspondingly great increase of effective light-power. These conditions exist in the large portrait lenses which were needed in the early days of photography to reduce the exposure time by collecting a great quantity of light from the object, and which in these days of rapid dry plates are no longer required for portrait work. Taking in some ten or twelve degrees of the sky, these lenses are specially suitable for photographing large surfaces such as are presented by the Milky Way.

This subject was taken up by the writer in the first part of 1889, at the Lick Observatory with a large 6-inch portrait lens of thirty-one inches focus, and with it was inaugurated the photography of the Milky Way. The first picture to show the real structure of the Milky Way was made in 1889, with this instrument. In the few following years a large series of photographs, of those portions of the Milky Way seen from the northern hemisphere, was made. The work with similar instruments was next taken up by Dr. Max Wolf, in Germany, who has also succeeded in making excellent pictures of the Milky Way. Mr. Russel, of Sydney, New South Wales, has also photographed portions of the southern part of the Milky Way with a large portrait lens. Those who have seen some of the Milky Way photographs taken with the regular Astro-Photographic telescope, or who have tried to make out its complex structure with a visual telescope, must be struck with the great beauty of a photograph made with one of these short-focus portrait lenses. The extraordinary complexity

of structure of the Milky Way is brought out with marvellous beauty of detail, and the peculiarities of its different portions can be traced and connected in the different photographs, which thus afford the most direct means for studying every feature of structure and detail. These pictures show many peculiarities which must materially alter our ideas of the constitution and structure of the Milky Way. Some of them show strong evidence that the general body of the Milky Way may be made up of small stars which are not at all comparable with our sun in dimensions. This is especially shown in the region of the star Rho Ophiuchi. Many parts of the Milky Way appear to be comparatively thin sheetings of stars with relatively no very great depth, for it is not possible otherwise to explain the black holes and rifts shown in them. One of the most important revelations made by the portrait lens in connection with the Milky Way, is the presence in it of very diffused nebulous matter apparently freely mixed with the groundwork of stars, and seemingly showing no definite tendency to condensation about the individual stars. These photographic nebulosities of the Milky Way are apparently of a different nature from the ordinary nebulæ of the sky, since they are extraordinarily large, diffused and but feebly luminous. These nebulous regions seem to be peculiar to the Milky Way and its vicinity, and are certainly in some way physically connected with it. It will be in the study by photography of such regions that we shall finally clear away some of the mysteries of the Milky Way. These masses of diffused nebulosity mainly affect regions of the sky in Scorpio, Cygnus, Cepheus, Perseus and Monoceros. I believe it to be true that no other form of telescope but the old-time portrait lens, or similar combination is capable of dealing with these extraordinary objects.

It was not until the study of the phenomena of comet tails with portrait lenses that we knew anything of the strange phenomena shown by them. It may be said that our knowledge of the extremely rapid transformations in the tails of comets dates from the photographs of Swift's Comet of 1892, taken at the Lick Observatory, with the lens previously mentioned, and by Professor Pickering at Arequipa with a similar instrument. Although the great comet of 1882 was successfully photographed, it showed no phenomena not known and already seen with the telescope. While only an insignificant affair visually, and but fairly visible to the

naked eye, Swift's comet showed upon the photographic plates the most extraordinary and rapid transformations yet seen in any comet. One day its tail would be separated into at least a dozen individual streams and the next present only two broad streamers, which a day later had again separated into numerous strands, with a great mass, apparently a secondary comet, appearing some distance back of the head in the main tail, with a system of tails of its own. This remarkable appearance was the first known of its kind, though it was repeated in the photographs of Rordame's Comet of 1893 made by Professor Hussey. These peculiar phenomena seem to be a production of the comet itself — a result of the forces at work in the head of the comet.

The photographs of Brooks' Comet of 1893, also secured with the Willard lens, showed such an extraordinary condition of change and distortion in the tail as to suggest some outside influence, such as the probable collision of the tail with some resisting medium, possibly a stream of meteors, such as we know exist in space. The long series of photographs obtained of this comet frequently showed great masses of cometary matter drifting away into space, probably to become meteor swarms. One of the pictures showed the tail of the comet streaming irregularly as if beating against a resisting medium and sharply bent at right angles near the end, as if at that point it encountered a stronger current of resistance. All of these wonderful phenomena would have been unknown to astronomers had it not been for these photographs, and the comet, instead of proving to be one of the most remarkable on record, would have passed without special notice. Though these phenomena were so conspicuously shown, scarcely any trace of the disturbance was visible with the telescope. On account of the apparent insignificance of the comet visually, no photographs were made of it elsewhere during its active period.

In the matter of discovery, the photographic plate has accomplished a very great amount in certain directions. In spectroscopic work, it has a field singularly suited to display its possibilities. In this direction it deals not alone with what can be seen, but it enters into the unseeable regions where the eye takes no cognizance of things; for, though it is partly blind to the light which affects the eye, it can readily penetrate regions where we in turn are blind. And it is in this direction mainly where the photographic discoveries in spectrum analysis are immediately concerned, since it

extends our vision into the invisible regions of the spectrum. The result must necessarily be one of discovery. It not only faithfully records spectral lines that can not otherwise be seen, but by special treatment of the plate it also registers those visible to the eye, and permits their accurate measurement.

From Doppler's principle, it is known that the spectral lines have a normal position only while the object whose light is under examination is motionless in the line of sight. When it is in motion to or from us, these lines are displaced from their normal position, in the first case towards the violet region of the spectrum, and in the other towards the red. By comparing any of the lines in the spectrum of a heavenly body with the same lines in the spectrum of a stationary object, it is possible to tell not only the direction of motion of the moving object, but to determine accurately the amount of this motion, for there is a known relation between the amount of displacement and the actual velocity, and this is independent of the distance.

This peculiarity, besides showing the motions of the individual stars, has revealed to us through the aid of photography an entirely new class of bodies, the so-called spectroscopic binaries.

The visual spectroscopic work, long ago, in the hands of Dr. Huggins, had shown the displacement of the spectral lines as the stars moved towards or from us. It remained, however, for photography to extend further this remarkable work by showing that not only were the lines displaced, but that in the case of certain of the stars the lines were periodically doubled at short intervals, thus indicating the presence of two bodies which must be rapidly revolving about each other; the doubling of the lines being due to the alternate approach of one and recession of the other body — which thus causes a displacement of the two sets of spectra, for when the motion is at right angles to the line of sight (and this must occur at two points in the orbit) the two spectra will be exactly superposed. It can readily be shown from the known periods of these stars and their enormous distances that no telescope is likely to be made so powerful as to show visually their independent components. The visual double star having the shortest period is one discovered by Burnham, and known as Kappa Pegasi, which he found to have a period of about eleven years. The spectroscopic binaries seem to revolve in extremely short periods — a few days — and in at least one case in a few hours,

showing that they must be extremely close to each other. The explanation, to account for the observed peculiarities of their spectra, that these are actual double stars in rapid orbital motion must be accepted until some better explanation of the phenomenon be forthcoming, which does not at present seem likely to occur.

Among the first of these spectroscopic binaries discovered was Beta Aurigæ which was detected at the Harvard College observatory by Miss Maury, through the doubling of its spectral lines as shown on the various photographs obtained of it at that observatory. This star has a period of four days, the relative motion of the components about each other being about one hundred and fifty miles a second, and the distance between them about six millions of miles. In a similar manner, Dr. Vogel has found that the star Algol — so famous for its light variations — alternately approaches us and recedes, in a manner that can only be explained at present by the revolution of that star about some other body or about the center of gravity of the two. The spectrum of this star does not show any doubling of the lines, but a simple displacement from one side to the other of their normal position occurs consistent with the changes of the star's light. As there is no doubling of the lines, the conclusion is that there is but one spectrum. One of the stars is therefore a non-luminous body, and hence produces no spectrum. The old explanation of two hundred years ago, that the variation in the light of Algol is due to a dark body revolving about it and partially eclipsing it at intervals of a little less than three days, is hence proved by the spectroscope and photography to be the correct one. The frequent discovery of these spectroscopic binaries shows that they are by no means uncommon, and that possibly a considerable percentage of the stars consists of two or more bodies rapidly whirling about each other.

The beautiful phenomenon of the displacement of the spectral lines through motion in the line of sight has given rise to many important and interesting results, but certainly none more striking than that offered by Professor Keeler's spectroscopic proof of the meteoric constitution of the rings of Saturn. It was suggested soon after the discovery of the rings that they must be made up of discrete particles revolving in zones about the planet, which from their smallness and great distance from us, gave the appearance of a system of solid rings encircling Saturn. This had been shown

by Clerk Maxwell to be a mathematical necessity, and as the rings lay within Roches' limit, within which a large solid body would be broken up in revolving about a planet by the unequal attraction of the planet itself, it was certain that the rings must consist of small individual bodies. It remained for the spectroscope, through the aid of photography, to add its testimony to that of mathematical analysis. The problem offered to the spectroscope was simply to show whether the inner or the outer portion of the rings moved the faster. Should they revolve as a solid body, the outer edge must necessarily have the greater velocity. But if they are made up of individual particles, then the attraction of the planet would cause those nearest to it to move the fastest, or in other words, the inner part of the rings must have the greater velocity. This beautiful problem was successfully solved by the photographs of the spectrum of the rings obtained by Professor Keeler, where the displacement of the spectral lines by motion in the line of sight showed that the inner portion of the rings moved faster than the outer, and hence that the rings must consist of small bodies responding individually to the attraction of the planet.

The discovery of variable stars by photography can be compared with the wholesale business in commercial circles, because of the great numbers that are found on the various plates. These stars are not only found by the actual variation of their light, as shown by the size of their images on different plates, but many of them also show peculiarities in their spectra, which at once stamps them as being members of a certain class of variable stars. So expert has Mrs. Fleming of the Harvard College Observatory become in detecting these bodies by their spectra that she instantly recognizes them at a glance among hundreds of others on the same plate.

The most interesting and important of these Harvard College variable star discoveries are found in the photographs of the globular clusters taken by Professor Bailey with the 13-inch telescope at Arequipa, Peru. It was found that a great many of the small stars, that make up these clusters, varied regularly and rapidly in their light and in some cases a large percentage of the entire mass of stars was variable. So abundant are these variables indeed, that as many as a hundred of them have been found in a space in the sky that would be covered by a pin's head held at the distance of distinct vision.

The clusters most prolific in variables are M 3, Omega Centauri, M 5, and a few others of this class. Perhaps the most remarkable circumstance, outside of the actual grouping of variables in such great numbers, is the fact that not a single variable star has been found in the great cluster of Hercules,[1] the best known of these objects, and apparently like them in all other respects. Professor Pickering finds every star in this cluster constant in its light from the photographic evidence extending through ten years. This would seem to mark this great cluster as being physically very different from the others referred to.

The writer has examined the cluster M5 with the great telescope of the Yerkes Observatory and has visually verified a number of these variables. The brighter of them appear to vary slowly in their light, while many of the smaller ones are extremely rapid, passing through their entire light changes in a few hours. In the discovery of such objects, photography offers special advantages; since on the different photographs a thousand or more stars can be rapidly and accurately compared with each other, and any variation in their light at once detected, while such comparisons in the actual sky, visually, would be limited to a very few stars. By the aid of the Harvard photographic plates, over five hundred variable stars have been discovered in these clusters. It must be said, however, in speaking of the variables in the cluster M 5, that the two most prominent ones were readily discovered visually nearly ten years ago by Mr. D. Packer, with a very small telescope. These two seem to have been the first of the variable stars found in this cluster.

The shortest period variable so far discovered in the globular clusters — indeed the shortest known variable — is a small star in the great southern cluster Omega Centauri, whose period is seven hours. These cluster variables seem to form a distinct class from the ordinary variable stars. It is very interesting to watch one of these small stars in a powerful telescope and to see with what quickness it passes through its light variation. One of the small stars in M 5, whose period is $12^h\ 31^m$, seems to be dormant for a large part of the time, as a very faint star, invisible in ordinary telescopes. It begins to brighten, and in two or three hours has risen nearly two magnitudes, and faded again to its normal con-

[1] Professor Bailey has since found that two of the stars of this cluster are slightly variable.

dition; while another and larger star, quite near it, seems to require a month or more to go through its light fluctuation.

Frequent reference has been made to the work of the Harvard College Observatory. It is to be regretted that time does not permit a more detailed account of this work. No other observatory is so active in the application of photography to the various departments of astronomy. Not content with the available sky as seen from the northern hemisphere, Professor Pickering wisely established a branch observatory at Arequipa, Peru, where a thorough photographic survey of the southern skies has been made, and a vast amount of work of high value has been accomplished, which has resulted in many important discoveries among the southern stars.

In dealing with the ordinary stars of the sky it has been shown that measures of the relative positions of the photographic images are strictly comparable with meridian circle work, while the number of stars that can be measured is vastly greater. The Pleiades, the cluster of Perseus, Præsepe in Cancer, etc., have all been measured with the micrometer, the heliometer and by photography. The comparisons have shown that photography has many advantages over the older methods, and the results are possibly even more accurate. These objects, however, are loose clusters and the stars are not thickly crowded, and moreover the small scale of the photographic plate in such cases does not seriously interfere with the work. The great globular clusters of the sky, however, from the extraordinarily crowded condition of their stars, would almost forbid any attempt to deal with the individual positions by photography, except in outlying regions, where the stars are thinly scattered. No comparison between photographic and visual measures of such objects has yet been made, because no visual measurements exist. The great cluster of Hercules is perhaps the easiest of these objects both visually and photographically. It requires, however, a powerful telescope to measure the individual stars. Dr. Scheiner has given a catalogue of 833 of the stars of this cluster, measured on photographs taken with the 13-inch refractor of the Potsdam Observatory. The stars that were measured all lie between the magnitudes 11.7 and 14. As a matter of comparison with visual measures, the writer has taken up the measurement of a few of the stars contained in Scheiner's catalogue. A rough inspection of the results so far obtained, shows a close agreement be-

tween the visual and the photographic work. These observations also show that no appreciable change has taken place in the positions of any of the stars in the past six years, which perhaps is surprising, since one would expect a possible rapid change in some of the positions of the individual stars where they are massed so closely together. They, however seem to be as stable in their relative positions as are the stars elsewhere in the sky. A more remarkable object with a great telescope is the cluster Messier 5, in which the stars are more closely compressed and irresolvable than in the cluster of Hercules. This object has already been mentioned in speaking of the variable stars discovered at the Harvard College Observatory. The measurement of nearly one hundred of these small stars has been undertaken with the great telescope of the Yerkes Observatory. Many of them are apparently in the very heart of the cluster where the compression is the greatest. It is doubtful if at this time photographs can be made of this cluster upon which the crowded individual stars in the center can be accurately measured. It has been frequently photographed, but no measures have been made of the great mass of stars in the center of the cluster.

It has been already stated that the accuracy of the photographic positions of the individual stars is perhaps as great as the best meridian observations. The facility and ease with which the photographic positions are obtained are well shown in a report by Professor H. H. Turner, who is making the Oxford portion of the great Astrographic catalogue. An average of 3,951 measures per week is obtained. Over one hundred and fifty stars per hour each can be measured by those most skilled in this work

In the discovery of nebulæ, variable stars and asteroids, the photographic plate has done a great work which is still being carried on. The number of known asteroids has been doubled in the past few years (as many as nine have been found in a single night) and now it has become a matter of impossibility to keep track of them all, and they are found and turned adrift again unless they show some striking peculiarity of orbit.

Up to the present time but two comets have been discovered by photography. The first of these was discovered on a photographic plate taken by the writer on October 12, 1892, with the 6-inch Willard lens of the Lick Observatory and was subsequently verified visually, and observed at the different observatories. The second

was discovered at the same observatory by Mr. Coddington with the same instrument, in July, 1898.

In photographing the sky, it is found that the short-focus portrait lens, from its small scale and large field, will show faint nebulosities beyond the reach of the larger photographic telescopes. This results from various causes. The action of these lenses upon the Milky Way, comets' tails and the great nebulosities of the sky, does not seem to be strictly subject in practice to the law of the ratio of aperture to focus; or, if it is, this law must be somewhat modified in effect. The action seems to be quicker with the short-focus lens than it should be. Probably, however, much of this is due to the small scale and the consequent compression of the image into a smaller space, which would produce an intensification of its action.. It is possible also that the photographic plate may be relatively more active with a bright image than with a faint one, which would give an advantage to the small relatively bright image of the portrait lens. This idea seems to be partly borne out by some experiments with a small lantern lens. This lens one and one-half inches in diameter and about five and one-half inches focus is much quicker than its light ratio would warrant, for it will photograph in a few minutes what the ordinary quick-acting portrait lens would require several hours to show. This was strikingly shown in photographs taken with it of the Milky Way. The scale of this lens is very small, and the cloud forms are so compressed that they act as a surface, and not as an aggregation of individual stars, as they must do in a larger telescope. If the focus is increased, the stars are scattered and the cloud no longer acts as a surface. With this small lens, the earth-lit portion of the new moon was readily photographed in a single second, while with a 6-inch portrait lens of one fifth, from twenty to thirty seconds were required to show it well. The brighter cloud forms of the Milky Way were shown in from ten to fifteen minutes' time, while with the larger lens upwards of three hours were required. Some of the diffused nebulosities of the Milky Way, notably in the region of Antares, are shown more quickly and more satisfactorily with this small lens, and a great wing-like nebula involving the star Nu Scorpio was discovered with it.

A list of discoveries made with these small lenses would be tedious; one of the most interesting, however, cannot be passed over because of its importance. There is no object in the entire

heavens better known than the great nebula of Orion. With the lantern lens, a great curved stream of nebulosity was shown on the plates of this region, covering a large portion of the constellation and some seventeen degrees long. It was found later that this had already been discovered by Professor Wm. H. Pickering with a two and a half inch lens in 1889. This object seems to be an outlying appendage of the great nebula. The discovery very much extends our knowledge of the complicated and far-reaching influence of this mysterious object. In several other cases the photographic plate has shown us that the nebulæ are far vaster than we had ever conceived them to be, for their fainter extensions are not seen by the eye. To what this knowledge may ultimately lead in the reconstruction of our ideas of space and its contents can hardly be anticipated just now, though it must, necessarily, very greatly influence those ideas.

We have spoken of the Pleiades and the entangling nebulosities shown by photography to involve the stars of the clusters. The portrait lens has shown us that not only are the individual stars of this group involved in a nebulous system, but that streams and masses of this filmy matter stretch out for great distances all about the cluster.

The photographic plate has shown itself especially adapted, when used with the rapid portrait lens, for the accurate registering of the paths of meteors, and it promises to be of special value during the expected return of the November meteors this year, when a more exact determination of the radiant will be obtained from the photographs and hence the orbit of the meteor stream will be better known.

In the reduction of the measures of the photographic plates for the great Cape Photographic Durchmusterung, Kapteyn discovered another "runaway" star with a proper motion of 8.71 seconds a year, which is much greater than that of the celebrated 1830 Groombridge, and is at present the largest proper motion of any known star.

There are very few departments of astronomy where photography has not taken a prominent, if not a commanding position. It is probable, however, that it will never take the place of the micrometer in the observation of close double stars and similar objects, and in this direction, the micrometer of Burnham will perhaps never be displaced. The photography of the surface features

of the planets is in an almost hopeless condition at present, yet much can be expected in this direction when an increased sensitiveness of the plates has been secured.

Photography has shown its value in the determination of stellar parallax and probably hereafter it will essentially take the place of the micrometer in this direction.

This is not the place to go into a discussion of the relative values of the refractor and reflector for photographic work. Where accurate measurement is to be considered, the refractor is doubtless better than the reflector. If, however, the main object is a great quantity of light such as is required for the photography of the nebulæ, the large aperture of the reflecting telescope of short focus makes it perhaps the best form of instrument, though it is very much hampered by its small field. This has been shown to be true by Common and Roberts. Since in the reflector the light does not pass through the glass, it is possible to use very large apertures without any additional loss of light through absorption, as would necessarily occur if it passed through a large object glass.

Mr. Ritchey, of the Yerkes Observatory, is making a large glass speculum, five feet in diameter and twenty-five-feet focus, which when finished will be one of the most powerful instruments for photographic and spectroscopic work yet made, and which deserves a more extended notice than my limited time will permit me to give it here. With this instrument, and Mr. Ritchey's skill in photographic work, results of high importance will be obtained.

Through the intelligent generosity of Miss Catherine W. Bruce, of New York City, astronomical photography has been placed on a firmer basis than it ever was before. Her gifts have been made to all departments of astronomy, and it would take considerable space to enumerate them all properly. Perhaps the most important of these are the ones that bear directly upon astronomical photography.

The first of these gifts was the great 24-inch photographic doublet by the late Alvan Clark, presented to the Harvard College Observatory, and which is now doing such excellent work in Peru; the two 15-inch portrait lenses for Dr. Max Wolf, of Germany, and a 10-inch photographic doublet for the Yerkes Observatory. These instruments are the most powerful of their kind, and for certain classes of work are superior to any other form of telescope. The results of the splendid gifts of this lady

must hereafter have the greatest influence upon the higher development of astronomical photography.

It is impossible within the limits of this address to give more than a general, and at best, incomplete sketch of the rise and progress of photography in the various lines of astronomical research. To those who have kept pace with these rapid strides in the last twenty years, this brief history will seem imperfect, and perhaps of little interest. Many applications of the photographic art, and many valuable results have necessarily been omitted. But few of the names of those prominently identified with this subject have been mentioned, and but little of their work even alluded to. A volume of no small dimensions would be necessary to give a complete history of the development of photography in the many directions in which it has been applied to astronomy. The time to do this has not yet come. Progress has been so rapid and far-reaching, that its history, however complete and exhaustive, a year later requires to be rewritten; and there is no reason for supposing that the end, or even the beginning of the end, has been reached. With new materials and new methods, and new workers who will profit by the experience and results gained by those who have in our time accomplished so much, we may expect for the new century far greater results than those briefly recorded here.

It would be difficult just here to predict the future of astronomical photography, though one can foresee something of the great results it must accomplish. It will displace some of the visual work, but it is more likely to move along new lines, opening up new fields of research. The older astronomy, so nobly represented by Simon Newcomb and a few others, will be strengthened at every point, and will stand all the more sublime for the help it shall receive from photography.

REPORTS READ.

THROUGH the efforts of the Acting Secretary and of the Sectional Committee of last year, four reports were prepared for the meeting and duly presented. By vote of the Council one of them is printed in full; the others are printed by title only, as they are to be published in full elsewhere.

REPORT ON THE RECENT PROGRESS OF SOLIDS AND FLUIDS. By Dr. ERNEST W. BROWN, Christ's College, Cambridge, Professor of Applied Mathematics, Haverford College, Pa.

Introductory.—In this paper I shall not attempt to give an exhaustive account of the progress which has been made in Hydrodynamics of recent years. Such an account, though possibly useful for purposes of reference, would be tedious and unsuitable for reading before an audience. I shall therefore try to give some idea of the general lines on which research has been carried on, laying stress on the more important discoveries and avoiding, as far as possible, mere technical details.

Range of the Report.—The choice of the period to be selected is not difficult. In 1846 Professor Stokes presented a report on the condition of Hydrodynamics at that time and this was continued by Professor Hicks in 1881–1882. Both these papers are printed in the reports of the British Association for the Advancement of Science. In the Mathematische Annalen for 1887, Mr. A. E. H. Love gave a summary of our knowledge of Vortex Motion and Hicks practically carried this to the present day in his Presidential Address before Section A of the British Association in 1896. Professor Darwin's Article on Tides in the Encyclopædia Britannica carries our knowledge of that subject to 1888. Hence I shall take the progress made in the general subject since 1882; the work on Tides will be taken from 1888 only, while Vortex Motion will be omitted altogether.

General remarks. — The application of Mathematics to the solution of many natural problems in fluid motion possesses one difficulty which is not common to most of the problems which confront physicists. It arises from the fact that we are frequently unable to apply the method of approximation. It usually happens that when a problem arises from some natural phenomenon, it is not capable of direct solution. But the

mathematician is generally able to consider a simpler problem which more or less closely corresponds to the given conditions. Having solved it, he is able to take into account the conditions of the actual problem and so to obtain a solution to any degree of accuracy which may be desired. This is frequently *not* the case with problems in fluid motion. The differential equations of motion may perhaps be written down but the limitations which have to be imposed before a solution can be discovered are so numerous that the solution, when found, often gives no approximation at all to the real circumstances. To illustrate this, we need only mention the case of a sphere moving through water. If we neglect the friction between the water and the surface of the sphere, the viscosity and compressibility of the water, it is easy to find all the circumstances of the motion. But when the velocity of the sphere is not small and we take these neglected circumstances into account, the motion, as any one will agree, quite changes its character. Instead of the stream lines (that is, the lines followed by the molecules of the fluid) being regular curves, eddies are formed all along the surface of the sphere, the motion in the rear of the sphere becomes turbulent and seems to defy all attempts at calculation. Further, the resistance to motion caused by the fluid, instead of being zero as in the simplified problem, actually becomes very large. And here, it is to be remembered, we are dealing with a simple case of a class of problems which has a high practical interest — the resistance experienced by a ship moving at a speed which we are accustomed to expect, say, from ten to twenty miles an hour. The engineer now knows fairly well the resistance by experiment. But neither he nor the mathematician can calculate the resistance when speed is forty miles an hour, and this speed has already become an accomplished fact.

Order of the Report. — I shall first deal with problems in which the motion is irrotational, that is, where the separate molecules of the fluid are not supposed to possess any rotation of their own independently of the rotation of the whole mass. In a fluid which is non-viscous such a molecular rotation can never be set up by any conservative system of forces. If then we neglect the viscosity and the skin friction of the solid with which it is in contact, all motions considered will be of the irrotational class. This class can be again divided into two others. First, that known as *continuous* in which the pressure never comes out to be negative, and, secondly, that known as *discontinuous*, in which we may have a negative pressure or a surface across which there may be a finite change of velocity. In the first case, the fluid completely occupies the spaces around the solid with which it is in contact. In the second case, hollows may be formed and there may be free surfaces, or the fluid in motion may be in contact with other fluid at rest. After treating these two classes of problems, I shall go on to mention the advances made in various kinds of Wave motion, including the Tides. Then will follow the motions and forms of masses of fluid rotating about an axis under their own gravitation only. Finally, the influence of viscosity will be considered.

Motion in two dimensions. The general problem. — We shall first consider those problems in which the fluid is incompressible and without viscosity and its motion is irrotational, steady and takes place in two dimensions. If we take the plane of motion to be that of $(x, y,)$ this class of course includes the case of motion in three dimensions where the motion of every particle in a straight line perpendicular to the plane of (x, y) is the same. The theory depends on the velocity-potential φ and the stream-function ψ. If u, v be the velocities of a particle of the fluid at the point $(x, y,)$ the following equations hold:—

$$u = \frac{\delta \varphi}{\delta x} \quad , \quad v = \frac{\delta \varphi}{\delta y} ,$$

$$u = -\frac{\delta \psi}{\delta x} \quad , \quad v = -\frac{\delta \psi}{\delta y} ,$$

$$\frac{\delta^2 \varphi}{\delta x^2} + \frac{\delta^2 \varphi}{\delta y^2} = 0 = \frac{\delta^2 \psi}{\delta x^2} + \frac{\delta^2 \psi}{\delta y^2}$$

Putting $z = x + i y$, $w = \varphi + i x$

these equations show that the determination of the motion reduces to the discovery of a function

$$w = f(z)$$

which will satisfy the given boundary conditions.

When w has been determined as a function of z, the curves,

$$w = \text{pure imaginary},$$

give the stream-lines, that is, the lines followed by the molecules of the fluid in their motion. The velocities are determined by partial differentiation with respect to x, y.

It is unnecessary to say more about the general problem. It is that of Divichlet in the Theory of Functions. Solutions can, in general, only be obtained by the inverse process of taking known solutions of $\frac{\delta^2 \varphi}{\delta x^2} + \frac{\delta^2 \varphi}{\delta y^2} = 0$ or $\frac{\delta^2 \psi}{\delta x^2} + \frac{\delta^2 \psi}{\delta y^2} = 0$, and inquiring what boundary conditions they can be made to satisfy. When the motion is discontinuous the boundaries, instead of being fixed walls, are free surfaces along which the velocity is constant.

As we are searching for the stream-lines

$$w = \text{pure imaginary}$$

that is, the lines parallel to the real axis in the w-plane, all that is necessary is to find the curves in the z-plane which correspond to lines parallel to the real axis in the w-plane. This problem is, therefore, reduced to one of conform representation. For walls consisting of straight lines a few cases have been long known, being solved by Schwarz's method. Some new ones with more complicated rectilinear boundaries were given by Réthy in 1895.

Recent work.—In 1890, Michell gave a new form to Schwarz's method which enables him to solve some further problems in discontinuous motion. These are mainly the cases of jets issuing from apertures into fluid which may be at rest or in motion. This was shortly afterwards modified and extended by Love. Still later B. Hopkinson added the case where several sources or sinks might be present. (These are points where the fluid is supposed to enter or leave in finite quantities.) The transformation does not in general contain infinities, but Hopkinson includes the case where $\frac{dw}{dz}$ becomes infinite like

$$\frac{A_1}{z-z_1} + \ldots . + \frac{A_n}{(z-z_1)^n}$$

Several hydrodynamical examples are given but the most interesting are the electrical applications.

These problems are of little practical value. They frequently demand the existence of dead water behind the moving solid. For example, if a rectangular board be drawn through a fluid, it is directly seen that the fluid behind the obstacle is far from being at rest, which the theory would indicate. Again, if water is entering into a large tank which is full, by means of a projecting tube in the side of the tank, the fluid instead of spreading in all directions (by the theory) actually moves like a fluid cylinder for a short distance.

In fact, the fluid, instead of moving back along the outside of the projecting tube, actually moves the other way. The difference is, of course, caused by the viscosity of the fluid which, even when small, produces vortex or eddy motion of a complicated kind. The difficulties have been stated with some detail and with several illustrations by Lord Kelvin in Nature, 1894.

Just lately, Professor Hele-Shaw has succeeded in photographing the movements of an actual fluid under similar circumstances. It is striking to observe that when the motion of the water takes place between two parallel plates not far apart and when its velocity is not very great, the stream-lines follow almost exactly the theoretical positions which they would have under the assumption made above. In the case of a rectangular plate held at an angle of 45° to a steady stream, the stream-lines agree almost exactly with the hyperbolas given by theory. When the parallel plates are not close, however, or the motion takes place in a tube of not too small section, any variation in the diameter of the tube soon produces turbulent motion.

Continuous motion in three dimensions.—When the motion takes place in three dimensions I divide it again into continuous and discontinuous motion. On neither is there much to say. When the motion is continuous and there are no forces acting on the solid which is moving through the fluid, the system of differential equations admits of three integrals and the integration is practically finished if we can find a fourth. Several continental writers have been considering, during the past two or three years, under what circumstances a fourth integral of specified form may

exist. Miss Fawcett has examined the case of the motion of a solid of the form known as an isotropic helicoid when gravity is the only force acting. The most interesting case is that where the solid starts from rest. The path followed by the centre of gravity is traced. The motion of an anchor ring when there is circulation through the aperture has been discussed by Greenhill, Basset and Dyson. The first-named has applied the σ-functions to the solution of the problem. Basset has also discussed the motion of a spherical bowl.

No problem of discontinuous motion in three dimensions has yet been solved, notwithstanding many unpublished attempts. I shall return to this subject later on.

Wave motion.—Coming next to the general theory of wave motion including the problem of the tides, two main classes may be distinguished. Waves of expansion belong primarily to the theory of sound and they will not be touched upon here. The second class concerns the different kinds of waves which our every-day experience of water in motion brings before us. It includes wave-motion on the large scale as exhibited by the tides, on a smaller scale in the long waves which are known under the name of a "ground-swell" in the Atlantic. Next the waves whose effects have the greatest destructive power—the short waves whose height is not very small compared with their length—the waves raised by the wind in a river, those which follow in the wake of a ship, the solitary wave travelling up a canal or in some rivers, known in England as a "bore." Finally the waves which are very small and which are mainly propagated by the surface tension of the fluid. These last are known as capillary waves and will only be mentioned incidentally. A general distinction of all these waves on the surface of water is made by mathematicians—the long waves where it appears permissible to neglect the vertical acceleration of the particles of the fluid without materially affecting the results, and the short waves where this vertical acceleration may not be neglected in the equations of motion. I shall, as far as possible, deal with them in this order.

Long waves.—The neglect of the vertical acceleration, which implies that the dynamical pressure is equal to the statical pressure, greatly simplifies the problem of wave-motion. Neglecting compressibility and viscosity, the ordinary problems of long-wave-motion are not very difficult. The motion of long waves in canals is an old problem and was used by Airy for explaining the tides. A paper by McCowan in 1892 on these long waves when the section of the canal is uniform must be mentioned. Usually the canal has been taken to be of rectangular section. McCowan treats one which has sloping sides. Incidentally he obtains a canal of such shape that these long free waves are propagated along it without change of form. But a more important result is the detection of a serious error in Airy's explanation of the double high-tide, sometimes observed in estuaries and rivers. The method adopted was, as usual, one of continued approximation and Airy fell into the error—not unknown to mathematical physicists—that of carrying his approximations further than the initial

assumptions warranted. It appeared that the wave might divide into two or even three waves. McCowan shows that this division of the wave is without foundation when the equations are correctly treated and that therefore Airy's explanation of the double high tide fails. The double high-tide is still unexplained.

Tides.—The general theory of tides is fully dealt with and brought up to date by Professor Darwin in his article in the Encyclopædia Britannica of date 1888. Two important papers have appeared since then. To report how these two papers have advanced our knowledge of tidal theory, a few remarks must be made on the general problem.

The theory of the tides is mainly one of forced oscillations. The sun and moon moving in orbits round the earth whose essential nature is periodic, their motions are expressed by means of sums of sines or cosines of angles which vary with the time, and the periods of these terms differ. The difference of the attractions on the earth and the water which covers it, produces oscillations with periods which correspond to those of the sun and moon. Hence the periods of the principal tides will be known in advance. Notwithstanding this fact, the general equations which express the motion of the water have hitherto only been integrated on the following assumptions: First, that the particles of water never move far from their mean position in comparison with the radius of the earth—an assumption which is easily justified and which need not be discussed.

Secondly, that the ocean covers the whole earth supposed spherical. Thirdly, that the depth of the ocean is uniform or a function of the latitude only. Fourthly, that the attraction of the ocean on itself is to be neglected. The last phrase simply means that we neglect the difference of the attraction of the water on itself in its actual form, from its attraction in the nearly spherical form it would assume if there were no disturbance.

The irregular shapes of the bed and shores of the ocean make calculation of the effects due to them almost impossible. But the attraction of the ocean on itself should be susceptible to calculation. Hitherto this has always been neglected, owing to mathematical difficulties. Poincaré and Hough, in two papers which have lately been published quite independently and nearly at the same time, have taken it into account. Of these two papers, Hough's is the more important in view of the applications.

Previous writers, and particularly Darwin, have used simple sums of harmonic terms. Hough uses Zonal Harmonics and finds that this enables him to include the attraction of the ocean on itself. In particular the fortnightly tide has to be altered from five to eight per cent, owing to this cause. Poincaré makes a rough statement only, which is double this amount. At the same time he points out that the irregular shape of the continents may alter the coëfficients of certain terms to a large extent. In consequence of this he criticises the arguments of Thomson and Tait, who attempt to explain the difference between theory and observation by the solid tides which the moon and sun must produce in a solid

earth possessing slight elasticity. He leaves the question open however. Poincaré's paper contains much more in the way of theoretical researches, which must be omitted here. Hough also determines the free oscillations by means of an infinite determinant.

Short waves.— When we do not neglect the vertical acceleration of the particles of fluid the mathematical treatment alters and becomes more difficult. It is in general supposed that the height of the wave is small in comparison with the distance between the crests. In 1883, Lord Rayleigh discussed the short waves which are seen in front of an object trailed along the water and also the longer waves that are left behind. His method is to find the effect produced by a line of disturbance inclined at an angle to the direction of steady motion of the fluid. The effects observed by Froude in his famous experiments on ship-waves are fairly well accounted for. This year a paper by Michell has just appeared in which he calculates the wave resistance due to a body shaped approximately like a ship. Again the results are not unsatisfactory. Lord Kelvin has solved a similar problem in several articles in the Philosophical Magazine for 1886–7. He considers the effect of small inequalities in the bed of a stream. Of special interest are the standing waves produced by a stone, or by a hole in the stream-bed or by a wavy bottom such as we sometimes see left on the seashore by the retreating tide.

When the wave-height, wave-length, and the depth of the fluid are comparable in magnitude, the problem becomes very difficult, as the waves are not in general propagated without change of shape. They were first treated by Stokes. In 1889, von Helmholz took up the general problem and considered the effect of the wind in making permanent waves. Michell has traced the free-wave in the case of infinite depth and its changes as it proceeds. Incidentally he finds again the case where the crest becomes a cusp with an angle of 120°, a result predicted by Stokes from simple considerations. A paper by W. Wien should be mentioned; in it the forms of the waves at the surface of separation of two media are discussed and forms for the waves produced by the winds, more or less closely approximating to actual phenomena are found. The wave-fronts are figured by means of Schwarz's method of representing conformably a lemniscate on a circle.

The solitary wave.— The solitary wave has received a fair share of attention. In this wave the height is not necessarily small compared with the depth of the fluid; and it may travel for a long distance along a uniform canal with little or no change of form. Experiments have been made and the results published by Scott Russell. Boussinesq and Lord Rayleigh have lately investigated his figures from the mathematical standpoint and have given a simple formula for the relation between the wave-length and the height which agrees with that deduced by Scott Russell from experiment. Korteweg and de Vries in 1895 extended the theory of the solitary wave and showed that a new type — named conoidal — can be propagated. They also proved that a depression without a corresponding elevation can move along, but such depressions are generally unstable.

McCowan and Stokes have also treated these waves. The existence of free oscillations in canals has been investigated by Greenhill, Lamb and MacDonald by Fourier's methods. There is some doubt about Lamb's results. MacDonald has shown that it is necessary to take into account the possibility of satisfying the surface conditions and that simple formulæ are not sufficient; this point will be readily appreciated. The criticism appears to be valid but the subject needs elucidation.

Capillary waves.—An interesting problem is obtained by considering the tiny waves formed in a glass of water, when the glass is made to vibrate by means of a violin bow drawn across its edge. Rayleigh has shown that they are due to capillary action. It is curious that the number of waves formed on the water in a given time is double the number of vibrations made by the glass in the same time.

Rotating masses of fluid.— A good deal of interest has been taken during the last two decades in the forms assumed by masses of fluid rotating about a fixed axis under their own attraction only. Darwin and Poincaré are mainly responsible for the developments which the subject has received although much has been done by Madame Kowalewsky, Basset, Dyson, Bryan and Love. To give an account of all this work would take me far outside the limits of this paper and I shall therefore simply mention some of the more interesting results obtained by the first two writers.

In 1886 Darwin worked out fully the various possible forms of the Jacobian ellipsoids, showing where the limits of stability came. In 1887 he took up the more general problem of two nearly spherical masses of fluid rotating like rigid bodies round a fixed axis under their own attractions only. This problem is of great importance in theories of cosmogony. Especial attention is paid to the cases where the two bodies get very near to one another. When quite close they may coalesce and form dumb bell shaped figures; or the smaller mass may have a tendency to break up into two parts as shown by a furrowing in its contour.

Poincaré took the subject up from a different point of view. He starts with the rotating ellipsoidal form and investigates what forms of relative equilibrium are possible when small deviations from the ellipsoid occur consistently with the conditions. He figures a pear-shaped form of possible equilibrium and also discusses with much detail the stability of the various figures.

I am indebted to Professor Darwin for a reference to a paper by Schwarzchild which has just appeared in the Annals of the Munich Observatory. In this Memoir certain portions of Poincaré's work, with respect to the exchange of stabilities between two classes of possible figures of equilibrium at the place where they meet, are criticised. He also examines the stability of Roche's Ellipsoids by means of Lamé's functions and shows that there is no figure of bifurcation in this series.

Viscosity.—A considerable amount of attention has been devoted to our last subject, the Viscosity of Fluids, partly owing to the mathematical interest of the subject and partly to the difficulty of obtaining any close approximation to the results afforded by natural phenomena. It must be

stated at the outset that in all the work hitherto attempted the motion is supposed to be sufficiently slow to enable us to neglect the squares and higher powers of the velocities. Practically this entails a serious limitation on the usefulness of the results. Even the most casual observation shows that when the velocity exceeds a limit which is very easily reached, eddies are formed and the motion entirely changes its character. Korteweg has made some suggestive remarks on this point in a paper on the Stability of the Motion of Viscous Fluids. He remarks that the existence and formation of eddies were generally supposed to be due to unstable solutions of the equations of motion. Von Helmholz, however, had found that when a solution with given boundary conditions in a simply-connected region is obtained, that solution is unique. Hence we cannot attribute the formation of eddies to other solutions which in fact do not exist. Korteweg further proves that this unique solution is stable. Hence, neglecting squares and products of the velocities, it is evident that having got a unique stable solution, eddies cannot be formed. Concerning these eddies, he says: — "When, on the contrary, squares and higher powers of the velocities are taken into account, I have my reasons for supposing that even in the case of a sphere moving with uniform velocity — if such a state of steady motion can be reached — the motion must finally become unstable." Lord Rayleigh has also attempted to examine how viscosity affects the motion, and how eddies, when formed, are maintained.

"Lord Kelvin concludes that the linear flow of a fluid through a pipe or of a stream over a flume bed is stable for very small disturbances, but that for disturbances of more than a certain amplitude the motion becomes unstable, the limits of stability being smaller, the smaller the viscosity." (Lamb's Hydrodynamics.) It is possible that a remark made by Klein in his lectures on the top may have some bearing in this case. He points out that near the limit of stability obtained as usual by neglecting the second and higher powers of small quantities, instability really takes place when we include them. It is indeed possible that all viscous fluid motions as at present investigated are really unstable and that eddies are always formed. It is instructive to read Reynolds' experiments of 1883 as to the point at which, with increasing velocity, stream-lines appear to break up into eddies.

A warning must be given against laying too great a stress on the equations of motion. They are formed under certain suppositions as to the character of the internal friction of the fluid, but we have no security that these suppositions represent the facts. At the same time, most of the different assumptions made lead to the same equations, so that only a very fundamental alteration would affect the equations of motion.

Another difficulty arises with the skin friction at the surface of a body moving in the fluid. The difficulty arises from the fact that the action along the wall in contact with the fluid is treated quite differently in the cases of no friction and very small friction. The mathematical character of the motion may be completely altered. The difference is this. With non-viscous fluids we assume a finite slip of the fluid along the wall. If

even the smallest coëfficient of friction be introduced a finite slip cannot be consistently allowed. The whole subject is very obscure. It is now believed by some prominent physicists that no such finite slip actually takes place at all.

A somewhat easier problem is the decay of waves proceeding along water, owing to the influence of viscosity. In several short papers during the last three years in the Comptes Rendus, Boussinesq has treated the various kind of waves — in many cases reducing his results to numbers. A practical application is a determination of the time necessary for the sea raised by a storm to subside. Hough has treated a similar problem, namely, the effects of viscosity on ocean currents and on tides of long period. He concludes that for big slow currents — such as the Gulf Stream — the friction of the ocean bed is by far the most important factor in the dissipation of the energy of motion, while for the short waves in deep water, viscosity becomes paramount. The continued existence of ocean currents is a problem not satisfactorily explained. They are usually attributed principally to the tendency of the winds to blow on the average mainly in one direction. Against this is urged the dissipation of the energy thus acquired, by the viscosity of the fluid. Hough concludes that too much effect has been attributed to viscosity. Such currents will doubtless take a long time to start but when once set in motion, the modulus of decay is so large that energy is dissipated very slowly and the winds are sufficient to supply the energy lost by viscosity. The long-period tides again are supposed to be greatly affected by viscosity If, however, Hough's conclusion that the modulus of decay is comparable more nearly with twenty years than with a few months, those tides whose periods are as great as one or even six months, will be but little affected. Hence the differences between observation and the results of the equilibrium theory of the tides, originally attributed to viscosity, cannot be explained in this way.

A few special problems of motions of solids have been solved, the squares of the velocities being neglected. The motion of a sphere and the linear motion of an ellipsoid in an infinite fluid had been solved. Edwardes in 1892 added the rotational motion of an ellipsoid and the motion of fluid through a channel bounded by an hyperboloid of revolution. As before stated, the results have little more than a mathematical interest.

Future Progress.—To attempt to give any idea of the possible directions in which future progress is likely to be made is a dangerous task. One can, however, do something by mentioning the problems in which a little progress has been made and also those which have been before the scientific world for some time and remain yet unsolved. For some of the indications given below, I am indebted to friends who have themselves contributed to recent progress.

Problems in discontinuous motion in two dimensions in an infinite, frictionless, incompressible fluid are not without fundamental difficulties. In fact they are mainly exercises in conform representation. The prob-

lem is reduced to that of finding a function to satisfy Laplace's equation for two dimensions with given boundary conditions. No special service will be gained for Hydrodynamics by solving for new forms of boundaries unless the cases arise in experiment. But I have mentioned the fact that no problem of discontinuous motion in three dimensions has yet been solved. The difficulty is one which can easily be appreciated. The theory of functions deals with a complex of the form $x + iy$, and this suits all problems in two dimensions. But little has been done with a vector in three dimensions and certainly nothing has been built up concerning it which corresponds to the results obtained for the two dimensional-vector Perhaps the paper on Potential by Sommerfeld in the Proceedings of the London Mathematical Society last year may have some bearing on the problem; it is in any case worth serious study. The subject of discontinuous motion was set for the Adams' prize in 1895—this is the prize which has produced Maxwell's essay on Saturn's rings and J. J. Thomson's on Vortex motion. A solution for a solid of revolution was asked for and it was generally supposed that the circular disc would be the easiest to attempt. No essay was sent in. One prominent mathematician who has aided considerably in the development of Hydrodynamics mentioned that he had worked for six months and had obtained absolutely nothing. A magnificent reception therefore awaits the first solution!

Some mention has been already made of difficulties awaiting solution in tide and wave problems. Whether much more can be done by the present analytical methods is not certain. We may frequently meet in this department with series which formally satisfy the equations, but which either converge very slowly or not at all. The theory of free and forced waves, depending to a large extent on methods of approximation, when numerical results are required for comparison with observation, is in many respects fairly satisfactory if the height of the wave crest above the mean level is not very great and if the motion is slow. But there are comparatively few problems solved for shorter waves in confined bodies of water. The oscillations of water across a canal of circular section is an instance of a problem which should yield to analysis. But little is known of the causes which produce double and other singular kinds of tides in enclosed areas.

Another class of problem is the wave-resistance experienced by various shaped bodies moving over water, especially when the velocity is not small. That this class is becoming extremely important in view of the high speeds attained by torpedo boats, there can be no doubt. The lines of a ship are at present a matter mainly of experience. One of the most puzzling things is the difference in the speed qualities of two ships built on the same plans. A very slight difference in the lines will frequently enormously alter the speed qualities and one can scarcely doubt that this is due to the differences in the waves which are formed as the ship moves. The kind of problem thus presented to the mathematician is always a difficult one. Briefly stated, it is a problem where a slight difference in the boundary conditions makes a large difference in the effects produced.

A similar difficulty occurs when one approaches the limits of stability of steady motion.

The treatment of viscosity and skin friction seems to be a subject lying nearer to one's grasp. The difference between no viscosity and small viscosity — namely the mathematical assumptions of finite slip and no finite slip — has already been pointed out; this requires further investigation. Again, the fact that in all problems hitherto solved, only the first powers of the velocities are taken into consideration seems to point to an opening for research. We cannot hope that, even should this single difficulty be overcome to a certain extent, the mechanical difficulties would be made much easier. But every step taken ought to lead to some further insight into the most intractable subject that mathematical physics presents. It must be admitted that the practical engineer has almost no use for any of the results obtained by the theory as it at present stands. It may be answered that pure science does not look to practical ends. This is perfectly true in general. But it can hardly be an argument in the case of Hydrodynamics, the very bases of which are assumptions which are supposed to approximate more or less closely to actual conditions.

Conclusion.—In conclusion, I cannot resist making, or rather repeating an appeal to our pure mathematicians to devote more attention to this subject. The literature is easily accessible. The English treatises of Lamb, Basset, and Thomson and Tait, will supply most of the needs of a student, while the works of Voight and Kirchhoff and the Handbuch der Physik of Winkelman may be used to supplement them. Again, Stokes' report of 1846 and that of Hicks in 1881–2 will furnish the main points in the history of the subject. On Vortex motion we have the later paper of Love in 1887 and the presidential address of Hicks to the British Association in 1896. On Tides, Darwin's article in the Encyclopædia Britannica brings our knowledge up to 1888. Most of the work done since 1882 is to be found in the columns of such easily accessible periodicals as the Philosophical Magazine, the Proceedings of the London Mathematical Society, and the Proceedings and Transactions of the Royal Society. A full index of titles of papers up to date with short abstracts is obtainable from the Jahrbuch über die Fortschritte der Mathematik and the Revue Semestrielle.

It is of course unfair to ask anyone successfully engaged on his or her own special line of research to leave it for doubtful profit in another; but such may be done by those who have the direction of the studies of the future generation by interesting and suggestive courses of lectures. Pure mathematicians will not find their knowledge useless here, and students will not be backward in following the footsteps of such men as Laplace, Stokes, Kelvin, Von Helmholz and Rayleigh. The tendency towards the separation of pure mathematics from their applications to physical problems has already been arrested on the European continent. It will not be a departure from American traditions if we do our share in this work.

REPORT ON THE RECENT PROGRESS IN THE MATHEMATICAL THEORY OF ELECTRICITY AND MAGNETISM. By Prof. ARTHUR G. WEBSTER, Clark University, Worcester, Mass.

REPORT ON THEORY OF INVARIANTS: THE CHIEF CONTRIBUTIONS OF A DECADE. By Prof. HENRY S. WHITE, Northwestern University, Evanston, Ill.

REPORT ON THE MODERN GROUP THEORY. By Dr. G. A. MILLER, Cornell University, Ithaca, N. Y.

PAPERS READ.

[ABSTRACTS.]

MAKING ASTRONOMY POPULAR. By Miss MARY PROCTOR, New York, N. Y.

MUCH has been done within the past few years in making astronomy popular, and bringing it within the reach of all, by means of popular works on the subjects, and illustrated lectures. The attendance at these lectures and the attention shown throughout prove that they are decidedly popular, and the members of the Association for the Advancement of Science deserve much praise for the work they have done in this direction. A knowledge of the wonders of the universe is ennobling, and by means of the New York City Free Lecture Course, these truths are now taught to mechanics and the poorer classes, and they cannot help being benefited by such knowledge.

CORRECTION OF LOCAL ERROR IN STELLAR PHOTOMETRY. By HENRY M. PARKHURST, Brooklyn, N. Y.

LOCAL error a necessary result of observation in zones.

Illustration by Proctor's D. M. chart.

Observations of asteroids furnish the means of connecting local error by comparing different regions with the same standard.

The standard is obtained by comparisons of asteroids with each other, each having been compared with many stars in different regions. A close agreement of several asteroids indicates that local errors have been "balanced out," and that these asteroids agree with the average standard of the Zone Catalogue (Harvard Annals, Vol. XXIV).

Having obtained a new standard, free from local errors, stars are compared with this new standard, rejecting all prior observations of them affecting the standard; and other asteroids are compared with these stars when passing near them.

This work was begun in January last and, up to May, twenty-four asteroids have been reduced to the new standard, together with several hundred stars, representing several thousand observations. (Explanation of mode of reduction in the use of old observations.)

Comparison of stars contained in the Harvard Zone Catalogue with the new standard, showing the *amount* of the local error in that catalogue, after eliminating the errors of observation from both values.

THE PRAESEPE GROUP; MEASUREMENT AND REDUCTION OF THE RUTHERFURD PHOTOGRAPHS. By FRANK SCHLESINGER, Ph.D., Columbia University, New York City.

[This paper will be printed in annals of The N. Y. Academy of Sciences.]

DISCORDANCES BETWEEN THE NORTH-POLAR DISTANCES OF STARS DERIVED FROM DIRECT AND FROM REFLECTION OBSERVATIONS. By J. R. EASTMAN, Professor of Mathematics, U. S. Navy, Washington, D. C.

THE systematic corrections to the observed north-polar distances of stars have been determined, since the beginning of the use of transit circles, by a method which has always given much trouble to astronomers. The method in question gave different results for stars on the north side of the zenith from those found for stars south of the zenith. This paper is a partial report of an investigation to discover the cause of these discrepancies, and it has resulted in a method for the treatment of reflection observations which makes the corrections to direct observations the same on both sides of the zenith and its use in the formation of the second Washington Star Catalogue has given greatly improved positions of the stars.

BEHAVIOR OF THE ATMOSPHERES OF GAS- AND VAPOR-GENERATING GLOBES IN CELESTIAL SPACE. By Dr. J. WOODBRIDGE DAVIS, Woodbridge School, New York City.

THE author considers a globe of artificial construction composed of asbestos fibres loosely laid together, the interstices filled with gas. At the centre is a furnace, and a system of tubing distributes the heat through the ball. As an effect of the heat thus supplied, the gas slowly exudes from the globe.

It is shown upon thermodynamic principles that, if the temperature of the exuded gas be equal to or greater than

$$\tau_0 = \frac{\delta D_1 r_0^2}{19900},$$

where δ is the relative density of the gas with respect to hydrogen, D_1 is the density of the globe, and r_0 is the radius of the globe in miles, the

intrinsic energy of the gas is sufficient to drive it in an outward flowing stream to an infinite distance against the force of gravity, and that the velocity in miles per second of the outflowing atmosphere at any distance r from the globe's centre is expressed in the formula

$$V^2 = V_o^2 + \frac{.011}{\delta}(\tau_o - \tau) - \frac{D_1 r_o^2}{1812000}\left\{1 - \frac{r_o}{r}\right\},$$

where V and τ are the velocity and absolute temperature at distance r, and V_o and τ_o are the velocity and absolute temperature at the globe's surface.

If the initial temperature is below that indicated, the atmosphere will not be able to flow away to infinity, but will be limited in height by the value of r found from the last equation when V and τ are made zero. The exudation will simply make this atmosphere more dense until its weight stops the exudation.

The behavior of a vapor-generating globe is the same except that the atmosphere becomes misty, presenting the appearance of a comet while still remote from the sun.

Some examples are given in illustration: a hydrogen-generating globe 1000 miles in diameter, of density equal to that of the sun, requires a temperature of only 17.7° absolute, or 256° C. below the freezing point of water, in order to be able to drive its atmosphere to infinity; a temperature of 1770° absolute is necessary for a similar globe 10,000 miles in diameter; the atmosphere of a 500-mile water-globe need decline in temperature only 28¼° C. in giving forth energy sufficient to blow the vapors to infinity; nine-eighths of a degree suffices for a 100-mile water-globe, and a decline of only 0.000113 of a degree serves to expel utterly the vapor from a globe of water or ice one mile in diameter.

A 5000-mile globe of solar density and exuding a mixture of three parts by volume of hydrogen and one part of oxygen at an initial temperature of 2,500° absolute, being assumed, it is shown that the resulting phenomena are much like some that present themselves in the heavens. When the temperature of the outrushing atmosphere has descended to about 1100° or 1200° absolute, the gases deflagrate, forming a striated spherical sheet of flame surrounding the globe. Immediately outside the flame-shell is a region of subincandescent gas, and beyond this the hydrogen and condensing water-vapor appear as an atmosphere of strongly radiated structure growing rarer outwardly to the limits of visibility. But, as the globe gradually cools, the velocity of the hydrogen diminishes until it can no longer blow away the mist. This remains suspended, balanced between gravity's down-pull and the fluid's out-push, until so great an accumulation occurs that its weight overcomes the resistance; huge masses plunge to the globe, and the atmosphere clarifies itself. The vapors rise again, and the cycle is repeated. From a distance the body appears to wax and wane in brightness. Little water and ice spheres revolving about this globe have their vapors brushed away by its outsweeping atmosphere in the form of long trains directed from the central body.

The formulæ show that the density or mass of the atmosphere depends upon the rate at which it is generated, but that its behavior after generation, that is, its velocity at any place, depends solely upon the initial temperature, and remains the same however rare the atmosphere may be.

No application is made to any real celestial body, the object of the paper being to develop formulæ, and determine the conditions necessary to exist in order that the atmosphere shall be outrushing. These are two: first, there must be a source of heat independent of the substances forming the atmosphere, corresponding to the furnaces in the artificial globes, to force exudation or vaporization, because the primeval condensation of these substances cannot generate sufficient heat to reverse the process and make them reëxpand; secondly, the temperature must be sufficiently high to render the generated atmosphere outrushing.

GRAPHICAL LOGIC. By ELLEN HAYES, Professor of Applied Mathematics, Wellesley College, Wellesley, Mass.

MILL'S fifth "canon of induction" is essentially a mathematical proposition and is easily presented by means of the Cartesian graph-conception. The ordinary axes of reference must be viewed as cause-effect axes which become specialized for any particular subject of investigation. In the upper right-hand quarter of the field surrounding the origin is a region which may be called the *field of observation;* it represents the investigator's limits of experiment. Exterior to the field of observation is the *field of inference.* The curve expressing the concomitant variations of two phenomena—whatever kind of curve it may be—is an observation curve throughout the field of observation. The fifth canon is nothing more or less than a statement that whatever properties the curve may be shown to possess within the observation field may be attributed to it in the inference field in both directions. The generic cause-effect curve possesses properties which must be shared in by specific curves, but these properties cannot be incorporated in the abstract equation of pure mathematics. $+(x, y) = 0$.

[This paper will be offered for publication in Science.]

A DESCRIPTION OF INSTANTANEOUS AZIMUTH AND ALTITUDE CHARTS OF THE HEAVENS. By F. W. COAR, Cambridge, Mass.

THESE charts, eight in number, are so constructed that the stars are represented as they actually appear to the observer, the zenith being in the center of the chart. Each chart contains the stars of a celestial hemisphere. The eight charts show the stars as they become visible at various seasons of the year.

ON RATIONAL RIGHT-ANGLED TRIANGLES. — II. By ARTEMAS MARTIN, U. S. Coast and Geodetic Survey Office, Washington, D. C.

N. B. The foregoing paper is a continuation of the paper read at the last meeting.

[This paper will be printed in the Mathematical Magazine.]

VARIATION OF LATITUDE AT N. Y. CITY AND THE CONSTANT OF ABERRATION FROM OBSERVATIONS WITH A ZENITH-TELESCOPE (TALCOTT'S METHOD) AT COLUMBIA UNIVERSITY OBSERVATORY. By JOHN K. REES, Professor of Astronomy, Columbia University Observatory, N. Y. City, Director, HAROLD JACOBY and H. S. DAVIS.

[This paper will be printed in Astronomical Journal.]

INSTRUCTION IN ELEMENTARY ASTRONOMY FROM OBSERVATIONS MADE BY THE STUDENT. By WM. MAXWELL REED, Assistant in Astronomy at Harvard College, Cambridge, Mass.

A SUMMARY OF PLANETARY WORK AT THE LOWELL OBSERVATORY AND THE CONDITIONS UNDER WHICH IT HAS BEEN PERFORMED. By A. E. DOUGLASS, Assistant in charge, Lowell Observatory, Flagstaff, Arizona.

OBSERVATIONS have been made as follows: Mercury and Venus have been found by Mr. Lowell to rotate once in their revolution about the sun, keeping the same face towards it. Many canals have been found on Mars in light and dark regions, seasonal changes have been watched and the projections on the terminator are interpreted to be clouds forming at sunset. Numerous diameter measures indicate the existence of a Martian twilight.

Vesta shows a polar compression of $\frac{1}{10}$ and Jupiter's first satellite has been found to have changed form and rotation period since 1892. All the satellites of Jupiter show well-marked detail. Uranus rotates nearly in the plane of its satellites' orbit and in the same direction as they do, once in $22^h\ 49^m$.

During the four months at Flagstaff the zenith seeing has been 7 on Pickering's scale of 10 (standardized), but working on successions of nights has been an advantage. We have also used low powers and small apertures.

ASTRONOMY IN SOUTHERN CALIFORNIA. By Dr. LEWIS SWIFT, Director of Lowe Observatory.

A BRIEF recital of a few facts regarding the Lowe Observatory and the work accomplished since it was built in the latter part of 1894.

THE CONDITION OF THE SURFACE OF THE PLANET JUPITER. By G. W. HOUGH, Professor of Astronomy, Northwestern University, Evanston, Ill. (Illustrated with lantern slides.)

THE YERKES OBSERVATORY AND ITS WORK. By GEORGE E. HALE, Director of the Yerkes Observatory, Williams Bay, Wis.

A BRIEF description of the buildings and instruments of the Yerkes Observatory, with an account of the work carried on during the past year. (Illustrated with lantern slides.)

ON THE DUPLEX BASE APPARATUS OF THE UNITED STATES COAST AND GEODETIC SURVEY. By WILLIAM EIMBECK, Assistant, U. S. Coast and Geodetic Survey, Washington, D. C.

ON THE DIMINUTION OF THE REFRACTION OF THE ATMOSPHERE WITH HEIGHT AND ITS EFFECT UPON TRIGONOMETRICALLY DETERMINED ELEVATIONS FROM RECIPROCAL ZENITH DISTANCES. By WILLIAM EIMBECK, Assistant, U. S. Coast and Geodetic Survey, Washington, D. C.

FIFTY YEARS OF AMERICAN GEODESY. By E. D. PRESTON, Assistant, U. S. Coast and Geodetic Survey, Washington, D. C.

THE U. S. Coast and Geodetic Survey was organized in 1807, and reconstructed in 1843. Since its reorganization there have been only fifty years of real activity. For five years its labors were interrupted by the civil war. During this half-century of existence it has expanded in proportion to the needs of the country. Its annual appropriation has in-

creased five-fold, while the personnel and results have developed in even a greater ratio. From a simple survey along a small portion of the eastern coast, the work has been extended to embrace a complete triangulation of the entire coast of the United States, with additional work in the interior. About 29,000 geographical positions have been accurately determined. These are distributed over 300,000 square miles of territory. Nearly 5,000 original field sheets are registered in the Archives at Washington. Activity has also been displayed in the domain of Physical Hydrography, Hypsometry, Magnetism and Gravity. The publications range through Astronomy, Mathematics, Geodesy and many kindred subjects. Over 1,000 new charts have been issued in the last eight years, and coast pilots have been kept up to date for the Atlantic, Pacific and Alaska coasts. Many improvements have been introduced, both in the construction of instruments and in the method of observing. Miscellaneous investigations have been carried on in various lines of research, such as variation of latitude, deep-sea exploration, solar eclipses and transits of Venus.

THE MASS AND MOMENTS OF INERTIA OF THE EARTH'S ATMOSPHERE. By R. S. WOODWARD, Professor of Mechanics, Columbia University, New York, N. Y.

THE object of this paper is to determine limits within which the mass and moments of inertia of the earth's atmosphere must lie. The conclusions are, using round numbers to express them, that the mass of the atmosphere is greater than $\frac{1}{1000000}$ and less than $\frac{1}{1760}$ of the entire mass of the earth; and that the moments of inertia of the atmosphere are greater than $\frac{1}{3855000}$ and less than $\frac{1}{700}$ of the corresponding moments of inertia of the entire earth.

[This paper will be printed in Astronomical Journal.]

TWO NEW FORMS OF APPARATUS FOR MEASURING THE ACCELERATION OF GRAVITY. By R. S. WOODWARD, Professor of Mechanics, Columbia University, New York, N. Y.

THIS paper proposed and indicated the theory of two new forms of vibrating apparatus which will afford precise determinations of the acceleration of gravity.

THE GRAVITATION CONSTANT AND THE MEAN DENSITY OF THE EARTH. By R. S. WOODWARD, Professor of Mechanics, Columbia University, New York, N. Y.

IT was shown by the author, in Astronomical Journal, No. 424, January 1898, that the product of the gravitational constant and the mean density of the earth is equal to 36797×10^{-11} in C. G. S. units.

The present paper gives an astronomical equivalent for this product, and shows that although we cannot find the sum of those constants we may hope to find their quotient.

The desirability of additional experiments to determine the gravitational constant is pointed out.

ILLUSTRATIONS OF THE COMITANT METHOD OF CONSTRUCTING THE IMAGINARY LOCI OF ANALYTIC GEOMETRY, SO AS TO RENDER THEIR PROPERTIES EVIDENT TO THE EYE. By FRANK H. LOUD, Professor of Mathematics, Colorado College, Colorado Springs, Colo.

ON THE OPERATION GROUPS OF ORDER 48 AND THOSE OF ORDER $2p^3{}_3$ p BEING ANY PRIME NUMBER. By G. A. MILLER, Ithaca, N. Y.

THERE are only two groups of order 48 that do not contain a subgroup of order 24. Each of these two groups includes sixteen conjugate subgroups of order 3 and only one of order 16. Both of these selfconjugate subgroups of order 16 are Abelian—the one contains 15 operators of order 2 while the other contains 12 operators of order 4 that do not have a common square. The total number of groups of order 48 is fifty-two. Five of them are Arbelian and one is Hamiltonian. Fourteen may be represented as the direct product of a group of order 16 and an operator of order 3.

When p is an odd prime number there are just fifteen groups of order $2p^3$. Three of them are Abelian but none is Hamiltonian. Ten contain an Abelian subgroup of order p^3 while there are only two that contain a cyclical subgroup of this order. Each of the fifteen groups of order $2p^3$ contains only one subgroup of order p^3 and five of these groups contain also only one subgroup of order 2. When $p = 2$ there are only fourteen groups of order $2p^3 = 16$. Five of these fourteen groups are Abelian and one of them is Hamiltonian.

[This paper will be offered to Quarterly Journal of Mathematics.]

ON THE AIM OF THE INTERNATIONAL SOCIETY FOR PROMOTING THE STUDY OF QUARTERNIONS AND ALLIED SYSTEMS OF MATHEMATICS. By ALEXANDER MACFARLANE, South Bethlehem, Pa.

THE paper contained a brief history of the movement, and a statement of the object of the society. It showed that the branch of mathematical science which it is proposed to promote is not narrow, but of wide extent and penetrating into the heart of mathematical analysis. It also discussed some of the methods by which it is proposed to accomplish the aim.

ON HARMONIC FUNCTIONS. By ROLLIN A. HARRIS, U. S. Coast and Geodetic Survey.

THIS paper deals chiefly with particular solutions of equations of the forms

$$\frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2} + \cdots = 0$$

and

$$\frac{d^2U}{dx^2} - \left(\frac{d^2U}{dy^2} + \frac{d^2U}{dz^2} + \cdots\right) = 0.$$

A number of theses or assertions, self-evident or nearly so, are laid down at the outset, and upon these many of the results which follow depend.

By expanding $\varphi(ax + \beta y + \gamma z + \ldots)$ in powers of $\beta y + \gamma z + \ldots$ in accordance with Taylor's theorem, solutions of the first of the above forms are obtained if $\alpha^2 + \beta^2 + \gamma^2 + \ldots = 0$, and of the second if $\alpha^2 - (\beta^2 + \gamma^2 + \ldots) = 0$. For the purpose of breaking up the solutions thus obtained into others still more special, the quantity $ax + \beta y + \gamma z + \ldots$ is treated as a complex variable of several dimensions; it is then written $ax + iby + jcz + \ldots$, where $i^2, j^2, \ldots = -1$, for the first form of equation, and $ax + iby + jcz + \ldots$, where $i^2, j^2, \ldots = +1$, for the second form. A variable of two dimensions yields two solutions (X, Y or u, v in the ordinary theory of functions when the first form of equation is considered), a three-dimensional variable yields four solutions, a four-dimensional variable yields eight. By these means any number of solutions may be obtained some of which apply to a vibrating cord, a vibrating square or rectangular drumhead and long waves in a square or rectangular tank of water, waves in canals with sloping sides, and to spherical harmonic functions.

By aid of the theses already referred to, certain expansions and properties pertaining to zonal harmonics, Laplace's coëfficients, tesseral harmonics, and Bessel's functions are brought out. One peculiarity of the treatment consists in the avoidance of transforming Laplace's equation into spherical or cylindrical coördinates.

For example, take the solution of Laplace's equation

$$\frac{1}{r} = \frac{1}{\sqrt{x^2+y^2+z^2}} = \frac{1}{x} - \frac{1}{2}\frac{y^2+z^2}{x^3} + \frac{1\cdot3}{2\cdot4}\frac{\left(y^2+z^2\right)^2}{x^5} - \frac{1\cdot3\cdot5}{2\cdot4\cdot6}\frac{\left(y^2+z^2\right)^3}{x^7} + \cdots$$

If, now, $X(x) = \frac{1}{x}$ we have

$$\frac{1}{r} = X(x) - \frac{1}{2}\frac{1}{2!}(y^2+z^2)X''(x) + \frac{1\cdot3}{2\cdot4}\frac{1}{4!}(y^2+z^2)^2 X^{iv}(x) - \cdots$$

If we insert this value of $\frac{1}{r}$ in Laplace's equation, clearly the entire coefficients of $X''(x)$, $X^{iv}(x)$, . . . will therein be severally zero, and so the equation will be satisfied regardless of the nature of the function X. We may therefore replace $X(x)$ by, say, $\sinh x$; we thus obtain

$$\sinh x\left[1-\frac{1}{2^2}(y^2+z^2)+\frac{1}{2^2 4^2}(y^2+z^2)^2-\cdots\right] \text{ or, } \sinh x\, J_0(\omega),$$

as a solution of Laplace's equation.

THE GENERAL THEORY OF ANHARMONICS. By EDGAR ODELL LOVETT, Princeton University.

THE paper shows how Lie's theory of the projective group may be made to yield Clifford's statement of the general theory of anharmonics and thus identifies two theories widely diverse in appearance. In conclusion a complete generalization of Clifford's theory for space of n dimensions is presented.

[The paper will be published in the current volume of the Proceedings of the London Mathematical Society.]

SOME NOTES ON "DIRECTION." By SAMUEL EDWARD WARREN, C.E., Newton, Mass.

THE above paper suggests delay in discarding the terms "direction" and "distance" in elementary geometry, until their use can be further discussed; shows how the idea of them may arise and of the three, which it calls "independent directions" (for reasons given), points the relation of "right angles" and "dimensions" to their directions, and proceeds principally to test certain definitions of straight and curved lines which avoid the use of the word "direction."

RIEMANN'S THETA FORMULA. By FRANKLIN A. BECHER, Milwaukee, Wis.

I. AN historical sketch of theta functions and development of Riemann's theta formula.

The germinal idea of these functions came from Gauss. He discovered them and their main properties, which he communicated to Jacobi. Abel and Jacobi made special discoveries which led to the theory of functions of double periodicity. Jacobi devoted much time to a thorough study of the theta functions upon which he finally succeeded in establishing a theory of elliptic functions. Riemann advanced the subject still further which led to the development of the theta formula. Professor Prym and Dr. Krozer have given this formula a more general form and use.

II. Prym's short method for deriving Riemann's theta formula.

A note and suggestion upon the above method for deriving the formula of the form (*i. e.* Prym's).

$$2^{\vartheta}[\eta]\,((2u'))\,\vartheta\,[\eta+\rho]\,((2v'))\text{ etc.} = \sum_{[\epsilon]}(-1)^{\sum_{\mu=1}^{\mu=p}(\epsilon\mu\eta' u + \epsilon'\mu\eta u)}\text{etc.}$$

III. Short method and extension.

A QUATERNARY AND A TERNARY LINEAR GROUP EACH SIMPLY ISOMORPHIC TO THE GENERALIZED MODULAR GROUP. By Dr. L. E. DICKSON, Berkeley, California.

THIS paper is an extract from an article on "The structure of linear groups with certain quadratic invariants," which has been presented to the London Mathematical Society. It determines the structure of the quaternary group in the Galois Field of order p^n, $p^n>2$, of substitutions leaving invariant the function $\xi_1 + \mu_1 + \xi_1\mu_1 + \xi_2\mu_2$ of the four indices. Among its quotient groups occurs one which is simply isomorphic to the group of linear fractional substitutions of determinant unity. A comparison is made with the similar results (published in the Bulletin of the American Mathematical Society for May, 1898) on the ternary orthogonal group.

[This paper will be printed in the Proceedings of the London Mathematical Society.]

LINEAR TRANSFORMATIONS IN FOUR DIMENSIONS. By ARTHUR S. HATHAWAY, Rose Polytechnic Institute, Terre Haute, Ind.

[To be published elsewhere.]

THE LIMITATIONS OF THE PRESENT SOLUTION OF THE TIDAL PROBLEM. By JOHN F. HAYFORD, Expert Computer and Geodesist, Coast and Geodetic Survey, Washington, D. C.

THE purpose of the paper is not to present anything new; but to call especial attention to a point in which the present tidal theory is very weak, and at which it is important to strengthen it because such strengthening is apt to lead to a decided advance in the efficiency of tidal theory. The tides which would be produced upon the earth by the action of the sun and moon if it were covered with a great and uniform depth of water,—and of which the theory has been successfully elaborated in the past,—are so radically modified in the actual case in nature by friction, by great varia-

tions in the depth of the water, and by reflection from shores, that in explaining the actual tides theory furnishes very little except the *periods* of the various harmonic components of the tides. In many bays and rivers in which there is a marked tide these effects predominate so largely that the direct action of the astronomical forces may be neglected. Here then, the influence of shores and bottom should be studied. In many regions there is an abundance of well established facts as to the actual tides (as well as to the soundings and shore line), to be used in a study of this question. Whoever succeeds in furnishing an *adequate* explanation of the observed facts in bays and rivers, will also incidentally explain many of the anomalous facts in regard to oceanic tides and will make a great advance in tidal theory.

ON THE APPLICATION OF THE PRISMATIC CAMERA TO A GENERAL INVESTIGATION OF THE SUN'S ATMOSPHERE IN STRATA AT ALL POINTS OF THE LIMB. By Prof. DANIEL P. TODD, Amherst, Mass.

RESULTS reached during the total eclipses of 1896 (Nova Zembla) and 1898 (India), by means of the prismatic camera, indicate the highly desirable character of that instrument in the equipment of future expeditions. But the application of this instrument should be much more extensive than hitherto. Heretofore, on account of location of these instruments on or near the line of central eclipse, we were enabled to investigate the character and depth of the reversing layer and chromosphere at but few specific points of the sun's limb. But by extending the method to include stations located all the way from the central line to the north and south limits of totality, the application of the peculiar and important objective prism becomes perfectly general, and the conditions of the layers of the sun's atmosphere may be investigated all the way round the sun's limb. This increases the possibilities of any eclipse many fold, because of the wider location of stations. Attention is also directed to the equal availability of annular eclipses with total ones for this significant research.

A TIDAL ABACUS. By ROLLIN A. HARRIS, U. S. Coast and Geodetic Survey.

THIS apparatus enables a person to make predictions of the tide by means of curves upon movable strips, drawn once for all at a given station. Since the curves represent the harmonic components of the tide, the results derived from their use should agree with those obtained from tide-predicting machines. The curves shown represented the tide at Port Townsend, Washington. General directions for their construction and use are given on pages 183 185 in the U. S. Coast and Geodetic Survey Report for 1894, but the apparatus is now exhibited for the first time.

A PROPOSED TIDAL ANALYZER. By ROLLIN A. HARRIS, U. S. Coast and Geodetic Survey.

THE object of this paper is to show the feasibility of constructing a machine such that if into it the hourly heights of the sea be entered once, they will be distributed and added in the various ways required in tidal work.

Nearly all of the many wheels used are of two kinds, — a fact which would be of great importance in its actual construction.

THE HARMONIC ANALYSIS OF HIGH AND LOW WATERS. By ROLLIN A. HARRIS, U. S. Coast and Geodetic Survey.

THE process consists essentially in supplying by interpolation, without drawing the tide curve, the hourly heights of the semidiurnal portion of the curve. These are summed and analyzed for the semidiurnal components as if they were true heights of the surface of the sea. Knowing from observation the times and heights of the actual high and low waters, and having all hourly heights of the semidiurnal tide supplied as just stated, the former heights when diminished by the corresponding values of the latter, give points on the diurnal curve. These are summed and analyzed for the diurnal components as if they were true heights of the surface of the sea.

THE TREATMENT OF RESULTS FROM REFLECTION OBSERVATIONS AT THE GREENWICH OBSERVATORY. By Prof. J. R. EASTMAN, Washington, D. C.

From the earliest use of the Transit Circle at Greenwich, the practice of considering the values of R.–D. as corrections to all observations of stars, either direct or reflected, has been carried out in the most rigorous manner. It has always been assumed that a part of the correction was a function of the zenith distance and continuous from N. to S. Certain expressions have been framed every year to represent as far as possible the observed values of R.–D. This paper gives the results of an examination of the Greenwich work for forty-two years and apparently shows that the Greenwich assumption in regard to the variation of the values of R.–D. in Z. D. is not borne out by the observation.

REPORT ON RECENT PROGRESS IN THE THEORY OF THE GROUPS OF A FINITE ORDER. By G. A. MILLER.

THE object of the report is to outline some of the recent progress with a view to obtaining a clear understanding of some problems which seem to offer suitable subjects for investigation. It is divided into four sec-

tions bearing the following headings, in order: solvable groups, simple groups, substitution groups, abstract groups.

During the last two years it has been shown that it is possible to prove the solvability of a group by the method which Lie employed to prove that a continuous group is integrable. This method is theoretically elegant, but our knowledge of the formation of commutators is still too imperfect to make it very useful in practical cases. Frobenius has been the most successful during recent years in adding new systems of groups to the groups that are known to be solvable.

The simple groups of a composite order are of extreme importance in the study of solvable groups. If there were no such simple groups every group would be solvable. A number of systems of simple groups have recently been discovered. The first of these was found by Moore by means of an extension of the ordinary concept of congruences.

Although the theory of substitution groups was first developed, yet there are numerous problems in this theory which remain to be investigated. One of the most difficult of these problems is the determination of the class of the primitive groups that do not include the alternating group. In his memoir crowned by the French Institute, with the great mathematical prize in 1896, Maillet made considerable progress towards the solution of this problem.

Cayley first called attention to the fact that a group should be defined by the law of combination of its operators. Such a group is called an abstract group. In these groups the generational relations and the definitions occupy a much more prominent place than in the theory of concrete groups. The results obtained in this way find more direct applications to some problems than those of substitution groups.

[The report is to be published in full, in the Bulletin of the American Mathematical Society.]

A DESCRIPTION OF THE ALTI-AZIMUTH INSTRUMENT RECENTLY CONSTRUCTED FOR THE U. S. NAVAL OBSERVATORY. By GEORGE A. HILL, U. S. Naval Observatory, Washington, D. C.

PERSONAL EQUATIONS DURING THE PAST CENTURY, A BRIEF SUMMARY. By TRUMAN HENRY SAFFORD, Professor of Astronomy, Williams College, Williamstown, Mass.

TRIANGLES WHOSE SIDES AND AREAS ARE REPRESENTED BY WHOLE MEMBERS. By TRUMAN HENRY SAFFORD, Professor of Astronomy, Williams College, Willliamstown, Mass.

THE WESTERN BOUNDARY SURVEY OF MARYLAND OF 1897. By DR. L. A. BAUER, University of Cincinnati, Cincinnati, Ohio.

[This paper will be printed in Reports of Maryland Geological Survey.]

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ADDRESS

BY

FRANK P. WHITMAN,

VICE PRESIDENT, AND CHAIRMAN OF SECTION B.

COLOR-VISION.

THE domain of physiological optics, formerly much frequented by students of physics, has of late been administered chiefly by psychologists. So far is this true that I have hesitated, in preparing an address upon a subject in this realm, lest I should be accused of passing entirely beyond that borderland which lies between us and our sister science, of trespassing in a foreign country, and risking international complications. Yet a subject which has owed its development to Newton and Young, Maxwell and Helmholtz, to mention no other names, can hardly be out of place here. The methods of investigation are largely those of the physicist, the phenomena attend every optical research, the results are of frequent physical application. Within the past few years, however, most of the work on color-vision has been done by other hands, and the results have not appeared in the physical journals. It seemed worth while, therefore, to review briefly the progress of scientific theory in this direction, and to sum up, so far as possible, the present state of our knowledge.

For our purpose we must go back as far as Sir Isaac Newton, to whom we owe the first definite and intelligible hypothesis as to the nature of color-vision.

"If at any time," he says, "I speak of Light and Rays as coloured or endued with Colours, I would be understood to speak not philosophically and properly, but grossly, and according to such

Conceptions as vulgar People in seeing all these Experiments would be apt to frame. For the Rays to speak properly are not coloured. In them is nothing else than a certain Power and Disposition to stir up a Sensation of this or that Colour."

"For as Sound in a Bell or musical String or other sounding Body is nothing but a trembling motion, and in the Air nothing but that Motion propagated from the Object, and in the Sensorium 'tis a Sense of that Motion under the form of Sound; so Colours in the Object are nothing but a Disposition to reflect this or that sort of Rays more copiously than the rest; in the Rays they are nothing but their dispositions to propagate this or that Motion into the Sensorium, and in the Sensorium they are Sensations of those Motions under the forms of Colours."[1]

Again, with greater definiteness, Newton writes: "To explain colours, I suppose that as bodies of various sizes, densities, or sensations, do by percussion or other action excite sounds of various tones, and consequently vibrations in the air of different bigness, so the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the ether . . . of various bigness; the biggest, strongest, or most potent rays, the largest vibrations; and others shorter, according to their bigness, strength, or power; and therefore the ends of the capillamenta of the optic nerve, which pave or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations, which vibrations (like those of sound in a trunk or trumpet) will run along the aqueous pores or crystalline pith of the capillamenta, through the optic nerve into the sensorium; and there, I suppose, affect the sense with various colours, according to their bigness and mixture; the biggest with the strongest colours, reds and yellows, the least with the weakest, blues and violets; the middle with green; and a confusion of all with white, much after the manner that in the sense of hearing, Nature makes use of aërial vibrations of several bignesses, to generate sounds of divers tones; for the analogy of Nature is to be observed."[2]

These passages are quoted—and several others might be added—to show that Newton ascribes no peculiar function or activity to the terminals of the optic nerve. They are set in vibration by the rays of light; their vibrations are transmitted to the higher regions

[1] Opticks: 3d edition, 1721, p. 108.
[2] Quoted by Young, Phil. Trans., 1802, p. 19.

of the sensorium, where they become sensations of light and color. As to the physical nature of the rays themselves, or the reason why they should excite in the nerve-fibres vibrations of different length, Newton makes no guess.

This is a definite theory of color-perception, and, as Rutherford has pointed out, one of great value, but presenting obvious difficulties.

Some of these difficulties led Thomas Young to that modified form of Newton's views, which in the famous Bakerian lecture, he describes as follows:—

"Since . . . , it is probable that the motion of the retina is rather of a vibratory than an undulatory nature, the frequency of the vibrations must be dependent on the constitution of this substance. Now, as it is almost impossible to conceive each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it becomes necessary to suppose the number limited, for instance to the three principal colours, red, yellow and blue . . . and that each of the particles is capable of being put in motion less or more forcibly, by undulations differing less or more from a perfect unison; . . . and each sensitive filament of the nerve may consist of three portions, one for each principal colour." Young, as is well known, afterward substituted green for yellow, in his triad of principal colors.

It is to be observed that the "particles" spoken of in the above quotation, as vibrating in unison with the undulations of light, are not to be considered as molecules, in the modern sense. He speaks of them as particles, then as sensitive filaments, and in a later paper on the same subject, as "sympathetic fibres." A supposition, such as has been urged, that Young is here speaking of molecular constitution, and anticipating the photochemical theories of our own day, is an anachronism. Young is thinking of the wave-theory of light, and of sympathetic vibrations, or, as we should say, of resonance. One who meant chemical decomposition would not speak of the shattered molecules as *sympathetic fibres*. Young, in fact, is not consciously proposing a new theory, much less one so startlingly different. He is simplifying Newton's, making for that purpose four hypotheses, three of which, as he remarks himself, "are literally parts of the more complicated Newtonian system." Young's theory seems to have attracted

little attention, until brought once more to public notice by Helmholtz, in 1860.

Helmholtz suggests one objection to the theory, and a modification to obviate the objection, but remarks that the essence of the theory does not lie in this or that special assumption, but in the fact that the color-sensations are conceived as compounded out of three entirely independent properties in the nerve-substance.

Therefore, he says, if only for the sake of clearness of representation, he uses Young's original and simple form of statement, ascribing the different color-sensations to three kinds of nerve-endings in the retina, sensitive respectively to red, green and violet light, the sensation of white to the equal excitation of all three sets of nerve-fibres, and color-blindness to the absence of one or more of the sets of such fibres. He puts into Young's words, however, an entirely different meaning, by classifying Young's hypothesis as "only a special application of the law of specific sense-activities." This statement, a most natural one to a pupil of Johann Mueller, changes the whole character of the theory, and makes it really a new one.

The hypothesis in this simple form met very well the conditions of normal vision, and the cases of color-blindness thus far studied. Difficulties soon arose. Color-blind, especially red-blind persons, persisted in calling the principal colors seen in the spectrum, yellow and blue, instead of green and blue, as they should have done if the red sensation were simply absent. It was long supposed that this was merely a question of naming the colors, and that those actually seen were green and blue. So lately as 1892 the report of the British Association committee on color-vision contains colored spectrum-plates, in which the spectrum as seen by a red-blind person is shown as composed of green and blue, while that seen by a green-blind person is formed of red and blue. Abney prefixes the same plate to his Tyndall lectures on color-vision, published in 1894. But in 1881, Hippel[1] and Holmgren[2] had examined the vision of a person, one of whose eyes was normal, the other typically color-blind. All the long-waved end of the spectrum was described as yellow, the sensation being tested by comparison with the normal eye. Other cases of color-blindness, arising from disease, corroborate this testimony. It is evident that a simple

[1] Arch. f. Ophthal. XXVI (2) p. 176, 1880; XXVII (3) p. 47, 1881.
[2] See Proc. Roy. Soc. No. 209.

absence of one of the fundamental sensations is not enough to account for the facts.

But more than the ordinary color-blindness comes into the question. It was discovered that the normal eye is color-blind by indirect vision, the condition gradually increasing from the central portion outward, and culminating in the periphery of the retina in absolute insensibility to color. The sensation of white cannot in this case be produced by a mixture of three color-sensations, because color-sensation does not exist.

In 1777 was described, in the Philosophical Transactions of the Royal Society, the well-known case of the shoemaker Harris, apparently the first recorded case of complete color-blindness. In 1871 Donders made a careful study of another case. So rare is this defect, however, that so assiduous an observer as Holmgren in 1877 had never seen it and doubted its existence, and in 1889 Helmholtz simply passes it by, saying that the eye so affected is always in other ways ailing and sickly, the implication being that the effect was so far from the ordinary and so pathological in character that it need not be considered. But by 1892 a sufficient number of such cases had been examined to establish the existence of this as a distinct type of color-vision, and one which must be accounted for.

A third, and even more striking departure from the simple conditions of the original theory was found in the remarkable change in the character of color-sensations in faint light, the spectrum becoming simply a colorless strip of graduated brightness.

We have then three different types of varying color sensibility:

1. In the eyes of different persons, beginning with the normal-eyed, passing through the variation studied by Rayleigh and Donders, which while recognizing all colors, yet in the character of the sensations makes a step toward green-blindness, then the two well-marked divisions of the color-blind, red- and green-blind, and finally the eye which perceives no color. These different classes are well marked, and few intermediate forms are found.

2. In the same eye, under differing degrees of brightness. As the spectrum gradually diminishes in brightness, the colors change. Red tends toward yellow, yellow toward green, green becomes bluish. All the colors vanish by degrees, red disappearing first, and the spectrum, still visible through the greater part of its length, appears without color. Here the intermediate stage is a

sort of red-blindness, but the final result is the same for all eyes, that is, the distribution of brightness for the normal and the color-blind eye appears to be precisely the same, a circumstance, as we shall see, of much importance.

3. In a single eye, passing from the center outward. The fovea and the zone surrounding it are sensitive to all colors. Outside of this the eye becomes gradually insensitive to color, the sensation of red and green disappearing first, afterwards yellow, and finally blue. The outer part of the retina is insensitive to all color, but its condition may be quite different from that of the eye of a totally color-blind person, or a normal eye in faint light.[1]

In attempting to account for these varied phenomena, the Helmholtz theory loses its striking simplicity. It is no longer possible to explain color-blindness by the absence of one or more sets of sensitive fibres, or the white seen by a totally color-blind retina, as compounded from the three fundamental sensations — in any ordinary way.

Helmholtz, in 1860, made the suggestion, which was afterward amplified by Fick and König, of a possible changeability in the character of the three fundamental sensations. Albert, in 1882, from a study of the color-changes in faint light, pointed out the direction and character of the corresponding sensation-changes.[2]

A method by which such changes might be brought about was not difficult to suggest. The idea of Newton and Young, of nerve-fibres vibrating in unison with the light-waves, had grown increasingly improbable as on the one hand the rate of vibration of light became known, and on the other, the maximum rapidity of vibration transmissible by a nerve-fibre, which is a quantity of an entirely different order. Hence most modern theorists have turned to photo-chemical action, and Helmholtz among the rest, although with characteristic caution, he advocates no particular form of light-effect, resting the claims of his theory, in the second edition of his great book as in the first, on the physical possibilities of representing all colors by means of three. But if we suppose red-sensitive, green-sensitive, and violet-sensitive molecules, there is no difficulty, when one thinks of modern photographic processes, in supposing that the green-sensitive substance, for example, may be so chemically changed as to coincide in character

[1] Von Kries, Centralblatt f. Physiologie, X. pp. 745-749, 1896.
[2] Albert, Wied. Ann. 16, p. 38, 1882.

with the red-sensitive. Each would send to the brain the impression of its own characteristic color, while each would be affected in exactly the same manner by any given light. In other words, the color curve, corresponding to the sensation of the fundamental green in the Young-Helmholtz theory, would be more or less perfectly superposed upon that of the fundamental red.

As a result, there would be no more sensation of green, or red, but only of their compound, yellow. Now this is just what a color-blind person sees, as Hippel and Holmgren showed in their long-neglected papers already referred to.

So peripheral color-blindness may be explained by a gradual approximation of the three color-substances, and a gradual superposition of the corresponding sensation-curves, until in the outer zone all three coincide.

But when we attempt to apply this suggestion to all the changes noted before, in ordinary color-blindness, peripheral color-blindness, and twilight color-blindness, as von Kries happily calls it, the shifting of the curves becomes so great and so various, that we realize that we are dealing no longer with a scientific theory, but with fanciful and arbitrary arrangement. As a first approximation, the Young-Helmholtz hypothesis is valuable, leading to simple and definite connections between great numbers of facts. As a detailed explanation of existing phenomena, it is unsatisfactory, and growing more so daily.

One thing is becoming steadily evident, that the sensation of brightness, perhaps also the sensation of white, must be accounted for in some other way than as a summation-effect of separate color-sensations. Another class of phenomena points yet more directly to this view. I refer to the discovery of Rood that the effect of sudden variations in brightness, the "flicker" sensation, is dependent on brightness alone, and not upon color. It is difficult for one who has seen how easily and definitely a red and a gray, or a red and a blue, can be compared in brightness by this method, to believe that brightness is not an independent sensation. The words of Helmholtz are worth quoting here. "As to myself," he says, "I have always the feeling, that in photometric comparison of different colors, it is a question, not of the comparison of one sort of magnitude, but of the combined effects of two, brightness and color, of which I cannot form a simple summation and of which I can give no scientific definition."

The strongest point of attack upon the Young-Helmholtz color-theory has in fact always been the explanation of the white sensation, and one of the principal advantages of the theory proposed by Hering in 1874 was the separation of the white-sensation, which he identified with the sensation of brightness, from special connection with the color-sensations, or rather his including it, upon the same level, in the list of his three sets of opposing colors, black-white, red-green, yellow-blue.

A photo-chemical substance is supposed to exist in the eye — where, it is not decided — which possesses the remarkable property that, if acted upon by light of one wave-length, its molecules are dissociated or dissimilated; if acted upon by light of another wave-length, they are built up or assimilated again. This substance exists in three forms: one is, let us say, dissimilated by red light, assimilated by green; one is similarly acted upon by yellow and blue, and one by white and black. There are thus six color-processes, arranged in three pairs. They are antagonistic in character, so that if red and green, or yellow and blue light, act on the retina at the same time and with equal strength, they neutralize each other and the sensation of color is completely destroyed. It is difficult to form a clear conception of these processes. Photo-chemical actions of a somewhat antagonistic character are well known, but the analogies between them and the visual processes are hardly close enough to be of great assistance.

The hypothesis, however difficult to conceive of in itself, is very definite, and explains as well as the Young-Helmholtz theory, the results of ordinary color-vision, and in respect to subjective phenomena, as contrast, or after-images, is much more satisfactory, in formal statement, at least.

But it fails equally, though for different reasons, to explain satisfactorily the more lately discovered or less evident phenomena of vision.

Complementary colors are regarded, on this theory, as mutually destructive, the one representing an assimilative, the other a dissimilative process. The white or gray which results from their combination is due to the action on the white-black visual substance, which was unperceived in the separate colors, being masked by their greater brilliancy, but becomes effective when they are neutralized by mixture. If two equal grays are formed, one let us say from red and green, the other from yellow and blue, they must

according to the theory contain equal amounts of white, and the colors in each are completely neutralized. They should therefore remain equal at all degrees of brightness. But Ebbinghaus,[1] by mixing spectral colors, and Mrs. Franklin,[2] with color-disks, have shown that this is not true. If they are made equal when bright, and the intensity gradually diminished, the red-green mixture greatly exceeds in brightness. If they are equated when of feeble intensity, and then made brighter, the yellow-blue mixture surpasses. König[3] has lately examined this with much elaboration, confirming these results, and showing also that the brightness of a gray obtained thus by mixture is always equal to the sum of the brightness of the separate colors, whether by bright or faint light.

This single experiment, so amply confirmed, appears completely destructive to Hering's fundamental hypothesis, at least in its original and simple form.

It may be remarked in passing that photometric methods which have been proposed, of comparing lights of different colors by reducing their brightness until the color-differences vanish, are worthless.

Other phenomena of similar character exist which are equally difficult of explanation. Hering regards some of these as phenomena of adaptation, and shows, in his remarkable paper on Purkinje's phenomenon, published in 1895, that the state of the retina, as conditioned by previous exposure to light, affects greatly the perception of color. Attention to this fact is necessary in photometric comparisons. The eye should be kept in the same condition, as nearly as possible, throughout any set of observations. But careful measurements by von Kries and others, keeping the eye carefully adapted for brightness, have proved that Purkinje's phenomenon exists, whatever the state of the eye, though much modified by adaptation.

Another cause of false color-appreciation, insisted upon by Hering, is the pigmentation of the macula. This is certainly of importance. In experiments with color-disks the apparatus, to secure consistent results, must always be placed at the same distance from the eye. A color-match made with the disks close to the eyes will in general not hold if the observer steps back a few feet, because

[1] Zeitschrift für Psychol. und Physiol. der Sinnesorgane, v. p. 145, 1893.
[2] Mind, N. S., II, p. 487, 1893.
[3] Ber. d. Preuss. Akad. p. 945, 1896.

the macula covers in the two cases a very different portion of the retinal image of the disks. The region corresponding to the macula, indeed, can generally be seen projected upon the surface of the revolving disks, as a spot inclining more to reddish than the remainder of the surface. The intensity of the yellow pigment, differing in the eyes of different people, must affect their general perception of color.

The well-marked divisions of the color-blind, into green-blind and red-blind, as they would be called in the Young-Helmholtz theory, were explained by Hering as due to the more or less deeply pigmented macula. But the utter inadequacy of this explanation has been abundantly shown.

Perhaps the most striking difference between the Hering hypothesis and the facts is shown in the distribution of color-sense in different parts of the retina. Ability to distinguish colors decreases gradually from the center to the exterior. The distinction of red and green disappears first, then the yellow becomes uncertain, and finally blue disappears, the outer zone of the retina being devoid of color-sense. The zones are not well defined, varying with the brightness of the light, and the size of the colored surface. But making due allowance for these circumstances, the area within which red is distinguishable differs from that occupied by green, and the yellow sensation differs in extent from the blue. If red and green, or yellow and blue, are due to the presence of the same visual substance, it seems that the boundaries should be co-extensive.

Even the sensation of white presents similar variations. There are, as has been already said, three cases in which the color-sense is wanting: the totally color-blind eye, the normal eye in faint light, and the periphery of the retina. The brilliant discovery of Hering, in 1891, that the distribution of brightness in the spectrum in the first two cases is the same, aroused great interest in the theory of the sensation of white, and went far toward establishing its position as a distinct and separate sensation. The third case, it was taken for granted, fell under the same law. But in 1896 von Kries showed that the distribution of brightness in the spectrum as seen by the outer zone of the retina is different, being practically the same as in the central portion, with its maximum in the yellow, and that the peripheral zone in the retina of a color-blind person shows the same deficient sensation for the longer wave-lengths, as in the color perceiving portions of the eye.

This is a matter of so much interest, that I have re-examined it with the flicker photometer, with results differing materially from those of von Kries. According to my experiments, the brightness of the colors of long wave-length diminishes continually, while that of the shorter wave-lengths increases continually, from the center of the visual field to its circumference. The conditions under which von Kries worked, however, were so different from mine, that I cannot regard my results so far as necessarily invalidating his. If his results are confirmed, they show that the sensation of white in the normal eye is not completely determined by the twilight sensation, or that of the totally color-blind. It contains elements derived from, or connected with, the mechanism producing the sensation of color, even in those portions of the retina where no color-sensation exists.

I have discussed these two theories somewhat at length, because our growth in knowledge of the facts of color-sensation has been conditioned largely by their existence. The enormous amount of work which has been done on the vision of the color-blind, on color-vision by varying illumination, on peripheral color-vision, not to mention researches upon more purely subjective phenomena, has been largely suggested by aspects of one or the other of these theories, or undertaken with a view to testing portions of them, and there has seemed no better method of exhibiting the results of these researches, than by placing them in connection with the hypotheses they were intended to test. I need hardly add that I have been greatly aided in this summing up by the polemical writings emanating from the hostile schools.

In this respect, at least, the two theories have been eminently useful, and have fulfilled one of the chief requirements of a scientific theory,— that its explanations can be tested by experiment. The earlier forms of color-theory suggested by Newton and by Young were hardly such. So long as the specific effect was conceived to be entirely in the central organ, to which the nerves merely communicated the vibrations of light, there was little upon which to base experimental work. Helmholtz, by ascribing the specific activity to the nerve-ending itself, made it necessary to describe this activity in some definite way, which could then be tested. The very simplicity of the conceptions of Helmholtz and Hering, at first the apparent guaranty of their truth, has proved their greatest value, but also their greatest difficulty.

It is not to be wondered at, that later theorists have attempted to modify one or the other of these hypotheses, instead of starting anew. Many such attempts have been made in the last few years, but few have attained more than a passing notice, and none any general acceptance. One or two, however, are of considerable intrinsic interest, and may command attention for a brief period.

Ebbinghaus attempts to advance a step upon the older theories by assigning to a particular retinal substance the function of color-stimulus. He finds this substance in the so-called visual purple, which was studied with great care by Kühne more than twenty years ago. This remarkable substance is reddish purple in its normal condition. On exposure to light it bleaches rapidly, passing through a series of tints until it becomes yellow. On further exposure to light it becomes colorless, but in the dark regains its original purplish tone directly without passing again through the series of changes in color. The color-stimulus is ascribed by Ebbinghaus to the absorption of light by the visual purple, and the character of the light-sensation is directly dependent on the color of the light absorbed, that is, upon the physical properties of the substance.

The purple substance, which is changed by the action of light into the "visual yellow," is identified by Ebbinghaus, in its two stages, with the "yellow-blue" substance of Hering. In its first stage it gives rise to the sensation of yellow, in the second stage to that of blue. The visual purple pertains to that element of the retinal complex known as rods. These are not present in the central portion of the retina, and the visual purple is apparently absent there also. But the fovea is sensitive to blue and yellow, as also to green and red. Ebbinghaus supposes that a red-green substance exists, even in the fovea, green in its first stage, red in its second; that the yellow-blue substance exists also in the fovea, but that the two, present there in about equal quantity, and nearly complementary in color, neutralize each other, leaving the fovea colorless. A white-sensitive substance is also supposed to exist, more sensitive to light than any of the colored substances, and thus we arrive at three sets of color-processes, similar to those of Hering. The two types of color-blindness are explained by reference to the fact that there are two kinds of visual purple, found in the eyes of different animals, one more relatively red in tone, the other inclining more to violet. The red-blind are supposed to

possess one of these, the green-blind the other. Certain anomalous and pathological color-sensations are supposed to be due to disturbances in the conducting nerves, or the central organs, and hence need not be fitted into the scheme thus outlined.

The physiological character of this theory, as Mrs. Franklin has shown, can probably not be sustained. It is difficult to believe that such a balance between the visual purple-yellow and the supposed visual red-green could exist, in all stages of both, that they would remain always complementary, and so the latter always invisible. Yellow light, according to this theory, should be most active upon the visual purple, but as a matter of fact, this material is bleached very slowly by sodium light, and indeed, König has shown that its maximum absorption is not in the yellow, but in the green. The visual purple exhibits other striking properties, of which the theory takes no account. It seems probable, on the whole, that the office of this substance is really a very different one, and that if it is concerned at all with vision, it is with the sensation of white light, not colored.

The theory of Ebbinghaus, then, if we deny its connection with the visual purple, rests upon the same basis as that of Hering,—a visual substance not identified, perhaps not discoverable, but recognizable only through the precision with which it explains phenomena, and the hypothesis itself becomes in the main a modification of Hering's, with the well-known pairs of photo-chemical substances, modified in their character so as to meet the facts more perfectly, removing some difficulties, but introducing others.

The chief advantage of the hypothesis for explanatory or speculative purposes lies in its greater freedom. The theory of Hering demands six color-processes. These are so connected together that they make not six, but three independent variables. Ebbinghaus so constructs his substances as to leave them nearly independent, the blue, for instance, no longer serving as the antagonistic substance to the yellow, but regarded as developed out of it, and possessing specific properties of its own. Under certain conditions the color-substances are supposed to neutralize each other, as with Hering, a supposition which adds greatly to the difficulty of the hypothesis.—but in general, five independent variables are at the command of the theorist which, it is evident, may be endowed with such various properties as to explain almost any conceivable difficulty of color-vision.

It may also be said that, with such an assortment of visual substances at command, the properties of which have at present no known chemical, physical, or physiological relations, but are deduced entirely from the sensations dependent upon them, the phenomena might probably be explained in an indefinite number of ways, and the different methods of explanation should be regarded rather as examples of ingenious speculation, than as real contributions to the advancement of science.

To such a category belong many of the later theories of vision. They incline to Helmholtz or to Hering according as their point of view is chiefly physical or psychological: for the standpoint of these two theories is fundamentally different.

Helmholtz, showing that all colors can be compounded from three, and that white may be also compounded, assumes that three color-sensations are sufficient, and that white may be regarded as a compound sensation. Hering, relying more upon the direct deliverances of consciousness, denies the compound nature of the sensation of white, and of yellow, whatever their physical composition may be, and says explicitly that "the entire separation of the optical nature of a light from the sensation which it arouses in us, is one of the most necessary prerequisites to a clear handling of the theory of color." Along the lines of these two theories, then, new hypotheses move, and will move, since each of them stands for something real, and has its own distinct advantages.

Upon a somewhat different basis rests a theory, hardly so much of color as of light-sensation, which was hinted at by various observers, but most clearly worked out by von Kries. This supposes that we possess two entirely distinct kinds of visual apparatus, one dependent upon the cones of the retina, the other upon the rods, and the visual purple connected with them.

Max Schultze, so long ago as 1866, mainly on anatomical grounds, suggested that the rods were probably the most important organs of vision in faint light. Animals which prey by night, as cats, moles, owls, etc., possess retinas rich in rods, but with cones either few or absent. Our own eyes perceive faint light more readily with the peripheral portions of the retina, where rods are numerous, than with the central portions, where they are few.

Helmholtz[1] pointed out the fact that if the visual purple is act-

[1] Physiol. Optik. 2d ed. p. 268.

ually connected with vision it must have to do with peripheral, rather than central vision, since it is absent from the fovea, and suggested that it might have to do with the perception of faint light.

In 1894, König studied the absorption curve of the visual purple, finding it substantially identical with the curve of brightness for the spectrum of low luminosity. Von Kries, combining these and other suggestions, considers the visual purple in the rods to be, in the human eye at least, the active agent for the perception of faint light. He shows that the phenomena of adaptation point in the same direction. In strong light the visual purple is soon bleached. An eye "adapted for brightness" is very deficient in power to perceive faint light. If it is now kept in darkness for about half an hour, this faculty is enormously increased. But in about the same period, the visual purple is practically restored. The essence of adaptation is the recovery of the visual purple. Red light, which does not act upon this substance, does not destroy the sensitiveness to faint light in an eye which has been exposed to it for even a considerable time.

If vision by faint light depends, wholly or partly, on the decomposition of the visual purple, and if light of long wave-lengths does not effect this decomposition, blue light when faint should appear much brighter than red, and Purkinje's phenomenon is thus easily explained. But in the fovea, where the rods and the purple are not present, this sensation of colorless faint light should not exist, and the color of any light bright enough to affect this portion of the retina at all, will at once be recognized. Von Kries declares this to be a fact. Two colors, equally bright in strong light, will remain so at all illuminations if their image falls entirely on the fovea; but, if not, the color which is of the shorter wave-length will in general be the brighter.

Vision by strong light, and color-vision, since both are possessed by the fovea, must be effected by the mechanisms of that retinal area, and these sensations von Kries attributes to the cones, which are supposed to be furnished with a tri-chromatic color-apparatus, and to afford the sensations of color and a compound sensation of white. If objection is made to the compound white, the details of this latter apparatus might be varied, might even approximate that of Hering's theory, without affecting the importance of the hypothesis, the essence of which is the two-fold nature of the sensation of brightness.

Such a theory explains easily the fact that grays compounded from different pairs of complementary colors, and equally bright in ordinary light, cease to be so in faint light. They are equalized at first by the cone-apparatus, and are seen in the faint light chiefly by the rod-apparatus, in which the scale of brightness is entirely different.

G. E. Müller makes the acute suggestion that the visual purple may not be a visual substance at all, properly speaking; but, while concerned chiefly with the phenomena of adaptation, may act also as a *sensibilisator*—to borrow a photographic term—for the white-sensitive substance, increasing its susceptibility in faint light. This modification of von Kries' hypothesis is perhaps simpler than the original and equally satisfactory.

Still another hypothesis for separating the white from the color-sensations is, that the sensation of white, from an evolutionary standpoint, was developed earlier than the sensations of color, and that the mechanisms of the latter are to be regarded as evolved from that of the fundamental sensation, and as modifications of it. Upon this idea Mrs. Franklin has founded her ingenious theory of light-sensation. Abney has made a similar suggestion, but in general terms only.

Such is a brief and hasty summary of the progress of color theory. We may well ask for the result. In the general shifting, what views have maintained or gained a footing? A few, I think, are fairly well established.

1. The number of color-sensations is small, and all color-theories positing a large number are to be distrusted. If experimental work is of any value whatever, it is certain that all light-sensations, for all purposes, may be expressed by a small number of variables. The Young-Helmholtz theory demands three. Hering's requirements, as Helmholtz has shown, may be expressed in terms of three, although the number of fundamental color-sensations, using color in its ordinary sense, is four. Such theories as those of von Kries and Mrs. Franklin require four variables, such as that of Ebbinghaus five. The introduction of a much larger number is gratuitous and unnecessary.

2. Out of this number of variables at least one is to be allotted to the white-sensation, or that which is closely akin to it, the sensation of brightness. It is no longer possible to think of white entirely as a compound sensation, however it may be compounded

physically. It is unnecessary to recapitulate the arguments for this statement, drawn largely from the three forms of total color-blindness.

3. White, however, can hardly be thought of as an entirely independent sensation. The phenomena of vision by faint light, the facts of peripheral vision, show that, under certain circumstances, color-sensations contribute their quota to the colorless one, and in differing amounts at differing brightness.

These phenomena are not satisfactorily handled by any of the principal theories. They are fairly well explained by the Helmholtz suggestion of shifting color-curves, nearly as well by the hypothesis of Hering and Hillebrand, that color-sensations possess specific brightening or darkening power, which makes itself more notable as the intensity increases. These are but formal explanations, however, and increase rather than diminish the difficulties of the theories to which they are attached.

4. The theory of von Kries, of different visual mechanisms for bright and faint light, supplements excellently the existing theories, and must be regarded as a distinct step in advance.

5. A definite and highly probable function has been assigned to the visual purple, the function of adaptation, and of causing or aiding vision in faint light.

Farther than these at present we can hardly go. The number and variety of known phenomena are great, and constantly increasing. Their inter-relations grow every day more complex, and the actual mechanism governing those relations still remains almost entirely unknown. Subjective experiment appears likely to yield little more aid. The various theories have arrived at such a state of perfection and, thanks to subsidiary hypotheses, to such a state of flexibility, that almost any visual result might probably be explainable by either. Perhaps the most hopeful line of research is that which, like König's study of the visual purple, seeks to find some relation between color-sensations and physical properties. Since so many phenomena point to photo-chemical changes in the eye, it would not be surprising if the next advance should come from the chemical side, rather than from the physiological, physical, or psychological, which have held the field so long.

REPORT READ.

REPORT OF THE COMMITTEE ON STANDARDS OF MEASUREMENT. By HENRY S. CARHART, Secretary, Ann Arbor, Mich.

THE determination of the mechanical equivalent of heat by the electrical method, as reported by Griffiths (Phil. Trans., A, 1893) and by Schuster and Gannon (Proc. Roy. Soc., Nov. 1894) gave a larger value than Rowland's corrected result. This fact has created a demand for the redetermination of the ampere in terms of the electro-chemical equivalent of silver. At the Toronto meeting of the British Association last year a grant was made to the B. A. Committee on Electrical Measurements for the purpose of carrying out this investigation.

At the Detroit meeting of this Association the grant of $50.00 previously made for the use of this Committee was made available for the past year. Though this appropriation was clearly insufficient for the purpose, it was decided that the redetermination of the ampere should be undertaken for the Committee of this Association in the physical laboratory at Ann Arbor. The work has been ably carried to completion by Professor Patterson and Dr. Guthe. The details of the method will be given in a paper by Dr. Guthe before Section B.

The discrepancy between Griffiths' results and those of Rowland is about one part in 400 at all temperatures between 15° and 25° on the nitrogen scale. Those of Schuster and Gannon exceed Rowland's at 19.°1 on the same scale by about one part in 500 (Johns Hopkins University Circulars, June, 1898). These differences exist after the final elaborate comparison of thermometers and the reductions to the same absolute scale of temperature.

Since the electrical methods employed to determine the mechanical equivalent of heat involve either the current and the E. M. F. of the Clark cell, or the square of this E. M. F., and since the E. M. F. of the Clark cell is determined by means of the silver voltameter, it is evident that the current enters the final result as the square. If the discrepancy is due entirely to an error in the value of the ampere, assuming the ohm to be correct, then the ampere should be one part in 1,000 to one part in 800 larger than the present accepted value. That is, the electro-chemical equivalent of silver should be increased from Lord Rayleigh's value of 0.001118 to 0.0011191 or 0.0011194. Lord Rayleigh does not claim for his result an accuracy greater than one part in 1,000.

The method used by Patterson and Guthe was that of a specially constructed electrodynamometer of large dimensions, and the employment of the torque of a phosphorbronze wire to equilibrate the counter-torque due to the effort between the magnetic fields of the stationary and movable coils. This method eliminates entirely the value of gravity g. The torque of the wire was measured by observing the period of vibration of a cylindrical brass weight of known mass and dimensions when suspended by the phosphorbronze wire. The entire success of this part of the investigation was due to the fact that the observations were made with the whole apparatus enclosed in a fairly good vacuum. Under these conditions the vibrations could be followed for hours at various intervals; the logarithmic decrement was almost entirely constant, and it was easy to obtain a curve connecting temperatures and periods of vibration as a torsional pendulum. The wire was so connected to the support and to the brass cylinder that it could be transferred from the vacuum apparatus to the electrodynamometer and back again without disconnecting it from the terminal pins. From personal inspection at the several stages of the investigation assurance can be given that the work has been most carefully executed at every point, and all known sources of error have been as completely eliminated as possible. The weights employed were compared with the standards at the U. S. Bureau of Weights and Measures in Washington; the standard of length was a half metre bar of speculum metal made for the University of Michigan by the late Professor W. A. Rogers. The time was taken from a standard Riefler clock checked by comparison with the observatory time. The result of the investigation is that the electro-chemical equivalent of a used solution of neutral silver nitrate, fifteen parts by weight of the silver salt to eighty-five parts of distilled water, is 0.0011192 gm. per ampere per sec. This exceeds Lord Rayleigh's value by about $\frac{1}{3}$ of one per cent. and causes the discrepancy in the mechanical equivalent of heat to disappear.

The corresponding change in the E. M. F. of the Clark cell will be from 1.4342 to 1.4327 at 15° C. A direct determination has not yet been made and this redetermination is reserved for the coming year.

Dr. Kahle has obtained for the electro-chemical equivalent of silver the value 0.0011182 (Wied. Annal., Vol. 59, p. 532) by the use of an electrodynamometer designed by von Helmholtz and a *fresh* solution of the salt. Pellat and Potier found the same value as that of Patterson and Guthe (Journ. de Phys. 9, p. 381, 1890).

REPORT ON THE STATE OF THE MATHEMATICAL THEORY OF ELECTRICITY AND MAGNETISM.[1] By ARTHUR GORDON WEBSTER, Clark University, Worcester, Mass.

IN considering the state of the mathematical theory of electricity and magnetism at the end of the first half-century of the existence of this Association, it seems hardly possible to avoid a comparison with the state of affairs at the beginning of that period. In 1848, strange as the statement may seem, most of the great discoveries in electricity had been made. Coulomb had by his remarkable quantitative experiments with the torsion-balance and the proof-plane set the law of inverse squares once for all upon its feet, and thus opened the way for the wonderful applications of the analysis of Laplace, Poisson, Gauss and Green. The experimental discovery by Oersted of the action of the electric current upon the magnetic needle had aroused the enthusiasm of Ampère, with the result of the swift production of his discovery of the laws of electrodynamics and their representation in mathematical form, by a process of reasoning characterized by Maxwell as "perfect in form and unassailable in accuracy." Within the next decade Faraday and Henry had independently discovered the phenomena of induction, and thus completed, with two exceptions, the list of epoch-making discoveries in the science. We may remark in parenthesis that the dynamo and the electric motor, which have wrought such a change in our city and Association in very recent years, were thus possible before the birth of the Association.

Not only had the quantitative laws of induction of currents been formulated by Faraday, but they had been obtained by a remarkable mathematical process by Neumann, using the then hardly recognized principle of the Conservation of Energy. The mathematical theory of the subject was accordingly in a well advanced state fifty years ago. It is to be noticed, however, that all the work then done had been on the basis of action at a distance, the existence of which was then unquestioned by mathematical physicists.

Not so, however, by that prince of experimental philosophers, Michael Faraday. Not less important for the theory of electricity than his discovery of current induction were his electro-statical researches by which he first showed that the forces between electrically charged bodies were not independent of the surrounding medium. Thus Faraday was led to concentrate his attention upon the medium instead of upon the charges themselves, and daringly to attack the notion of action at a distance. In order to clothe his ideas in an intuitional geometrical form, Faraday introduced the idea of physical lines of force, an idea that was long in having its fruitfulness recognized by others.

It was not until 1861 that the note was struck which has produced such a remarkable change in the theory of fifty years since. In Maxwell's

[1] As noted on p. 53, this Report is one of the four which were prepared for the meeting and by vote of the Council to be printed in full. The MSS. was received too late for insertion in Sec. A.

papers on Physical Lines of Force in the Philosophical Magazine of that year he gave Faraday's ideas a mathematical garb, and introduced to the mathematical world the theory that the energy was resident in the medium, rather than in the charged bodies. In 1865 appeared in the Philosophical Transactions Maxwell's chef-d'œuvre, the elaborate development of his ideas in his paper on A Dynamical Theory of the Electromagnetic Field. Here we find for the first time the application of Lagrange's dynamical equations to obtain in a systematic and logical way the laws of electricity and its connection with magnetism. Besides the notion of the localization of energy, both electric and magnetic, in the medium, we find the other idea, foreign to the old theory, of the magnetic action due to time-variations of the electric field, these variations being termed by Maxwell displacement currents. It is not a little remarkable that Faraday had considered the possible changes taking place in the electrical state of the dielectric medium by changes of the magnetic field, and had attempted to make them experimentally evident, but without success. Whether Faraday contemplated the effect of the changes of the electrical state on the magnetic field I am not able to state. At any rate the introduction of this hypothetical magnetic effect of the displacement currents by Maxwell gave rise to a hitherto unlooked-for possibility, namely, the establishment of an electro-magnetic theory of light. This theory not only enjoyed the advantage of novelty, but was free from the fundamental difficulties of the previous dynamical theories of light, in that in it no longitudinal wave appeared.

If Maxwell's theory was true, then experimenters should discover the magnetic effect of the displacement current. We may well imagine that for many years investigators were devising means to accomplish this result; but if this was so, they were not rewarded by success. However, a crucial question had been agitated, for according to Maxwell electrical and magnetic disturbances must be propagated with a finite velocity, and the theory of action at a distance must be doomed.

Before passing to the post-Maxwellian, or, as we may call it, the modern era, it may be convenient to state Maxwell's theory as he left it.

Electrical charges are related, not to the so-called field intensity, whose components are X, Y, Z, derivable from a potential Ψ,

$$(1) \qquad X = -\frac{\delta \Psi}{\delta x}, \quad Y = -\frac{\delta \Psi}{\delta y}, \quad Z = -\frac{\delta \Psi}{\delta z},$$

but to a new vector, called the electric displacement, and denoting the state of polarization discovered by Faraday. The components of this, f, g, h, are proportioned to the components of the field,

$$(2) \qquad f = \frac{K}{4\pi} X, \quad g = \frac{K}{4\pi} Y, \quad h = \frac{K}{4\pi} Z,$$

where K measures a physical property of the medium, Faraday's specific inductive capacity.

The density of the charge then is derived from the displacement by the equation

$$\rho = \frac{\delta f}{\delta x} + \frac{\delta g}{\delta y} + \frac{\delta h}{\delta z} . \tag{3}$$

Similarly in magnetism we are to consider two vectors, the field α, β, γ, derivable from a potential Ω,

$$\alpha = -\frac{\delta \Omega}{\delta x} , \quad \beta = -\frac{\delta \Omega}{\delta y} , \quad \gamma = -\frac{\delta \Omega}{\delta z} , \tag{4}$$

and the induction, proportional to it,

$$a = \mu \alpha, \qquad b = \mu \beta, \qquad c = \mu \gamma, \tag{5}$$

where μ measures another physical property of the medium. Maxwell here puts however

$$\frac{\delta a}{\delta x} + \frac{\delta b}{\delta y} + \frac{\delta c}{\delta z} = 0 \tag{6}$$

so that the analogy with the electrical equation (3) is not quite perfect.

To the equations previously accepted giving the relations between the current density u, v, w, and the magnetic field produced by it,

$$\begin{aligned} 4\pi u &= \frac{\delta \gamma}{\delta y} - \frac{\delta \beta}{\delta z} , \\ 4\pi v &= \frac{\delta \alpha}{\delta z} - \frac{\delta \gamma}{\delta x} , \\ 4\pi w &= \frac{\delta \beta}{\delta x} - \frac{\delta \alpha}{\delta y} , \end{aligned} \tag{7}$$

Maxwell adds the effect of the displacement currents, so that he has

$$\begin{aligned} 4\pi \left(\frac{\delta f}{\delta t} + u\right) &= \frac{\delta \gamma}{\delta y} - \frac{\delta \beta}{\delta z} \\ 4\pi \left(\frac{\delta g}{\delta t} + v\right) &= \frac{\delta \alpha}{\delta z} - \frac{\delta \gamma}{\delta x} \\ 4\pi \left(\frac{\delta h}{\delta t} + w\right) &= \frac{\delta \beta}{\delta x} - \frac{\delta \alpha}{\delta y} \end{aligned} \tag{8}$$

The induced electromotive-forces due to changes in the magnetic field are represented by Maxwell in a somewhat peculiar manner, as the negative time derivatives of a new vector called the vector-potential, so that

$$P = -\frac{\delta F}{\delta t}, \quad Q = -\frac{\delta G}{\delta t}, \quad R = -\frac{\delta H}{\delta t}, \tag{9}$$

The vector-potential is related to the magnetic induction by the equations

$$\begin{aligned} a &= \frac{\delta H}{\delta y} - \frac{\delta G}{\delta z}, \\ b &= \frac{\delta F}{\delta z} - \frac{\delta H}{\delta x}, \\ c &= \frac{\delta G}{\delta x} - \frac{\delta F}{\delta y}. \end{aligned} \tag{10}$$

Thus the vector-potential itself does not appear, but only its time derivatives.

The vector potential was introduced by Maxwell to denote what Faraday termed the electro-tonic state of a body undergoing induction of current by magnetic changes.

Strangely, as it now seems, the ideas of Maxwell were slow in gaining acceptation. We must not omit to notice that in 1867 an electro-magnetic theory of light was developed by Lorentz, but it was deduced from different considerations. It was not until the appearance of Maxwell's treatise, in 1873, that the attention of continental thinkers seems to have been drawn to the new views. Already, however, had begun the appearance of that series of papers by the master hand of Helmholtz, which, beginning with a powerful exposition of the old electrodynamical theories, led by successive steps to the development of a theory similar to that of Maxwell which, as the life of the great German drew to a close, became completely adopted by him. Otherwise, however, there is little to chronicle on the continent, until the appearance of the treatise of Mascart and Joubert, over a decade later than Maxwell's, showed that the seed had not fallen upon stony ground. Let us accordingly return to England, whither the center of gravity of the mathematical development of the subject was now transferred.

The first paper to appear in the Philosophical Transactions on Maxwell's theory was fifteen years later, by Fitzgerald, in 1880, on the Electromagnetic Theory of the Reflection and Refraction of Light. Here we find an extension of the suggestion made by Maxwell that the magnetic energy of the field contains terms depending on vortical motions about the lines of magnetic force, and thus an attempt is made to explain the phenomena of reflection of light from the surface of magnets, discovered by Kerr.

In 1881 followed a paper by Niven on The Induction of Currents in Infinite Plates and Spherical Shells, the former being a subject which had been investigated by Maxwell. In 1883 the subject of electrical

motions in spherical conductors was treated by Lamb who, however, makes the simplifying hypothesis that the velocity of propagation of the inducing effect is infinite, that hypothesis not materially conflicting with any experiments then made. In 1884 appeared a paper by Larmor on the same subject.

In 1884 appeared a very important paper by Poynting on The Transfer of Energy in the Electro-magnetic Field, where, by a direct application of Maxwell's theory, it was shown how the route taken by the energy in its passage from one place to another during variations of the field could be described by the statement that the energy flowed everywhere perpendicularly both to the electric and magnetic field-vectors; at a rate proportional to the area of the parallelogram constructed on their geometric representatives. Thus, according to Poynting, the energy passes from a dynamo to a motor not through the wires connecting the two, but through the air, being guided in its course, however, by the wires. The ideas of Poynting were extended by Wien in two papers in Wiedemann's Annalen in 1892, as well as in a paper by J. J. Thomson on Faraday Tubes of Force.

In 1885 we have a still more important paper by J. J. Thomson, on The Application of Dynamics to Physical Phenomena, in which, not especially the electrical ideas of Maxwell are developed, but rather the method introduced by him of applying Lagrange's equations is extended to a great number of phenomena. Singularly enough the same sort of methods were soon to be used by Helmholtz quite independently for similar purposes.

In 1887 Lamb attacked the more difficult problem than that of the sphere of ellipsoidal current-sheets, treating as a special case the flow of currents in a circular disc, and in 1888 the problem of induction of currents in shells of small thickness was treated by Burbury. The flow of current in cylindrical conductors has been treated by J. J. Thomson and Lord Rayleigh. In all these researches upon current-flow, interesting questions regarding partial differential equations were treated, but much is still left for the mathematician to do. In fact it was in this same year, 1888, that a striking sensation was produced in the scientific world by the publication of the experimental researches of a new genius, Heinrich Hertz, who produced in the laboratory the electrical waves conceived by Maxwell, and for the first time confirmed the theory by demonstrating the finite velocity of propagation. Experimental papers succeeded one another with astonishing rapidity, confirming one point after another of the theory, and the experiments of Hertz immediately obtained a vogue which has not yet subsided. The energies of mathematicians were now taxed anew, for the question of the nature and period of the electrical motions in the curiously shaped "oscillators" used by Hertz and his followers to produce the waves became very important. The previous investigations already described did not cover the case even for spheres and ellipsoids for, in the case of vibrations of the rapidity now experimentally realized, the effect of

displacement currents could no longer be neglected, nor could the velocity of propagation be considered infinite, while the phenomenon of radiation of energy from the conductor into space demanded mathematical recognition and treatment. This it failed to get, except in an approximate manner, for anything except the simplest case, that of the sphere. This was treated by J. J. Thomson and Poincaré. Nevertheless, the theory has not been experimentally verified for the sphere, because the sphere is a badly shaped conductor for experimental purposes. In order to retain energy enough to maintain the vibrations for a number of oscillations, the conductor should be long or even dumb-bell shaped, and not at all like a sphere.

The long spheroid is then next to be attacked, and then other surfaces, perhaps those obtained by the revolution of the curves known as cyclides. The introduction of suitable curvilinear coördinates into the partial differential equation concerned,

$$\frac{\delta^2 \varphi}{\delta t^2} = a^2 \, \Delta \, \varphi$$

will lead, even in the case of the spheroid, to new linear differential equations analogous to but more complicated than Lamé's, and will necessitate the investigation of new functions and developments in series.

The remarkable experimental skill shown by Hertz is not his only title to our admiration. His inaugural dissertation had been a treatment of the flow of electricity in spheres, and his electrical researches received a fitting completion in two mathematical articles in which Maxwell's theory was systematized and stated in an extremely clear and symmetrical manner. The equations of Maxwell, stated above, are unfortunately lacking in symmetry, and certain questions that have since arisen were not contemplated by him. These improvements were made in a very satisfactory manner by Hertz, as we shall describe later. We can not, however, award to Hertz the credit of priority in this matter, for the work had been already done by another writer, of whom I must now speak at length,—I mean that extraordinary Englishman, Mr Oliver Heaviside. Of this undoubted genius I feel that it is no exaggeration to say that he understands the theory of electricity probably better than anyone else now living. Of a decidedly eccentric personality and mode of expression, unknown to and unseen by most of his scientific countrymen, this self-taught luminary appeared on the horizon over twenty years ago, and slowly but surely approached the brilliancy of a star of the first magnitude. Unnoticed at first, he forced himself upon the attention of physicists by the sheer quantity of his productions, and it was then found that their quality was also exceptional. His writings have mainly dealt with a quite different sort of subject from those enumerated above, and have treated either the flow of variable currents in wires or the transmission of electromagnetic waves in free space. As early as 1876, in a paper modestly entitled On the Extra-current, he gives for the first time the partial differential equations for the propagation of current and potential along wires

and treats them by the methods of Fourier. Later he considers the most complicated questions caused by the terminal conditions involved by the introduction of various sorts of electrical apparatus such as those used in telegraphy and telephony. These papers will well repay the attention of pure mathematicians, to whom they offer a host of questions for rigid treatment. For instance, to fix the ideas, we have the equation of propagation

$$a\frac{\delta^2\varphi}{\delta t^2} + b\frac{\delta\varphi}{\delta t} = c\frac{\delta^2\varphi}{\delta x^2}$$

with the condition that for two or more given values of x, φ is to satisfy given linear *ordinary* differential equations in t, and that for a given value of t, φ and $\frac{\delta\varphi}{\delta t}$ are to be given functions of x.

If we attempt to satisfy the equation by particular solutions which are trigonometric functions of the time, we get an ordinary differential equation in x, and if we then make use of trigonometric functions of multiples of x, the multiples allowable will be determined by certain transcendental equations according to the terminal conditions. The function φ is then to be developed in a series of such trigonometric terms. The nature of the proofs desired relating to the series may be readily inferred. The papers by Heaviside are extremely numerous and bulky, and it is desirable that the methods there used should receive critical attention from mathematicians, for it must be said that Heaviside uniformly disdains such things as existence-theorems, depending chiefly on his intuitions drawn from physical reasoning.

A large portion of Heaviside's labors has been devoted to the systematization and extension of Maxwell's theory, and the attempt to disseminate a knowledge of that theory among the physical public. His results agree so nearly with those of Hertz that I shall give them in the notation and form used by the latter, which seem to me preferable. The attempt to bring out the symmetry, or reciprocity between electrical and magnetic phenomena, has been paramount with both Heaviside and Hertz. Accordingly, we have for the connections between the electric and magnetic field-intensities, represented respectively by X, Y, Z, and L, M, N, and the corresponding inductions, or polarizations $\mathfrak{X}$, $\mathfrak{Y}$, $\mathfrak{Z}$, and $\mathfrak{L}$, $\mathfrak{M}$, $\mathfrak{N}$,

$$\begin{aligned} \mathfrak{X} &= \varepsilon X \\ \mathfrak{Y} &= \varepsilon Y \\ \mathfrak{Z} &= \varepsilon Z \end{aligned} \qquad (11) \qquad \begin{aligned} \mathfrak{L} &= \mu L \\ \mathfrak{M} &= \mu M \\ \mathfrak{N} &= \mu N \end{aligned} \qquad (12)$$

Instead of equations (2) and (5), ε representing the specific inductive

capacity, and μ the magnetic permeability. For the electrical and magnetic densities ρ_e and ρ_m we have

$$\rho_e = \frac{1}{4\pi}\left\{\frac{\delta \mathfrak{X}}{\delta x} + \frac{\delta \mathfrak{Y}}{\delta y} + \frac{\delta \mathfrak{Z}}{\delta z}\right\}, \tag{13}$$

$$\rho_m = \frac{1}{4\pi}\left\{\frac{\delta \mathfrak{L}}{\delta x} + \frac{\delta \mathfrak{M}}{\delta y} + \frac{\delta \mathfrak{N}}{\delta z}\right\}. \tag{14}$$

For the mutual connections of the two fields we have

$$\left.\begin{aligned} 4\pi u + \frac{\delta \mathfrak{X}}{\delta t} &= \frac{\delta N}{\delta y} - \frac{\delta M}{\delta z}, \\ 4\pi v + \frac{\delta \mathfrak{Y}}{\delta t} &= \frac{\delta L}{\delta z} - \frac{\delta N}{\delta x}, \\ 4\pi w + \frac{\delta \mathfrak{Z}}{\delta t} &= \frac{\delta M}{\delta x} - \frac{\delta L}{\delta y}, \\ -\frac{\delta \mathfrak{L}}{\delta t} &= \frac{\delta Z}{\delta y} - \frac{\delta Y}{\delta z}, \\ -\frac{\delta \mathfrak{M}}{\delta t} &= \frac{\delta X}{\delta z} - \frac{\delta Z}{\delta x}, \\ -\frac{\delta \mathfrak{N}}{\delta t} &= \frac{\delta Y}{\delta x} - \frac{\delta X}{\delta y}. \end{aligned}\right\} \tag{15, 16}$$

Thus, with the exception of the electrical currents u, v, w on the left of equation (15) and the negative sign in (16), we have complete analogy between the electrical and magnetic equations. In these equations neither the electric nor magnetic potentials nor the vector potentials appear, and we are concerned only with the two field-intensities, which have a more tangible existence than the potentials. While equations (15) are identical with (8), equations (16) take the place of (9) and (10), for if we differentiate equations (10) according to the time, and substitute on the right for the time-derivatives of F, G, H, their values from (9), we obtain (16).

In order to obtain the result of propagation with finite velocity, let us consider a non-conductor, where u, v, w are zero, and let us suppose ϵ, μ

to be constants. Differentiating the third of equations (16) according to y and subtracting from the second differentiated according to z gives

$$\frac{\delta}{\delta t}\left\{\frac{\delta \mathfrak{N}}{\delta y}-\frac{\delta \mathfrak{M}}{\delta z}\right\}=\frac{\delta^2 X}{\delta x^2}+\frac{\delta^2 X}{\delta y^2}+\frac{\delta^2 X}{\delta z^2}-\frac{\delta}{\delta x}\left\{\frac{\delta X}{\delta x}+\frac{\delta Y}{\delta y}+\frac{\delta Z}{\delta z}\right\}.$$

Substituting the value of the parenthesis in the left from the first of equation (15), after making use of equations (11) and (12) and assuming that the parenthesis on the right vanishes, since there is no free electricity, gives

$$\varepsilon\,\mu\,\frac{\delta^2 X}{\delta t^2}=\frac{\delta^2 X}{\delta x^2}+\frac{\delta^2 \gamma}{\delta y^2}+\frac{\delta^2 Z}{\delta z^2},$$

which is the equation for the propagation of waves, as we have it in the theories of sound and light. The velocity of propagation is $\frac{1}{\sqrt{\varepsilon\mu}}$.

The last paper of the series of Hertz treats of the equations to be used in connection with moving bodies, and besides introducing terms accounting for the remarkable discovery by Rowland of the magnetic effect of a moving charge of electricity suggests several matters not yet verified by experiment. Still deeper into the theory goes an elaborate paper by Heaviside on the Forces, Stresses and Fluxes of Energy in the Electromagnetic Field, published in the Phil. Trans. of 1892. In this paper Heaviside deals with matters which are still open to controversy.

The last contributions of Helmholtz to the theory were his paper on the Principle of Least Action in Electrodynamics, published in 1893, in which he seeks to deduce all the electrical equations from this fundamental mechanical principle, and his paper on the Electromagnetic Theory of Dispersion, in which he adapts his beautiful explanation of this complicated optical phenomenon to the electromagnetic theory.

I have time here only to mention researches on electro- and magnetostriction, or change of form and shape of bodies in electric or magnetic fields, to which contributions have been made by Helmholtz, Boltzmann, Kirchhoff, Stefan, Lorberg, Adler, Cantone and Duhem, and on the magnetic effects produced by the motion of electrical charges, upon which subject papers have appeared by J. J. Thomson, Heaviside and Searle.

Before closing I must, however, mention several elaborate papers by Larmor, begun in the Philosophical Transactions for 1894, in which the attempt is made to propound a dynamical theory of the ether, which shall not only give a suitable explanation of light, but also a dynamical theory of all electric and magnetic phenomena, including the electromagnetic theory of light. For this purpose the old theory of McCullagh is found to be available, and is developed with extremely interesting results, while a great variety of phenomena are dealt with.

I have here made no mention of the work that has been done on the various theories of the mutual actions at a distance of current elements, as these are thoroughly dealt with in J. J. Thomson's admirable British Association Report on electrical theories in 1885. I have thus, in a very brief and unsatisfactory manner, merely touched upon some of the principal points of the development of the theory of electricity, and traced its gradual but unceasing progress from the hands of the giants of the old days to those of the new.

PAPERS READ.

[ABSTRACTS.]

THE MEASUREMENT OF THERMAL CONDUCTIVITY IN IRON. By Prof. E. H. HALL, Cambridge, Mass.

THE paper gives an account of further experiments made in the same general method described by me in an article printed in 1896 in the Proceedings of the American Academy of Arts and Sciences, under the title "On the Thermal Conductivity of Mild Steel."

ON ENERGY AND ENTROPY. By Prof. L. A. BAUER, Cincinnati, Ohio.

SECOND REPORT ON THE MAGNETIC SURVEY OF MARYLAND. By Prof. L. A. BAUER, Cincinnati, Ohio.

ON THE MAGNETIC DEFLECTION OF DIFFUSELY REFLECTED KATHODE RAYS. By Prof. ERNEST MERRITT, Ithaca, N. Y.

REFLECTED kathode rays were discovered by Goldstein in 1881 and their general characteristics were studied by him at that time. Since the discovery of X-rays, however, some doubt has arisen as to whether the phenomena observed by Goldstein were really due to reflection, and it has been suggested that they indicate the existence of some new kind of ray intermediate between kathode rays and X-rays. Prof. S. P. Thompson has held the view that these rays, which he called "para-kathodic rays," were in some respects distinctly different from the kathode rays which produced them. The experiments here described were undertaken to determine quantitatively the magnetic deflection of the para-kathodic rays. If it should appear that these rays are deflected in the same manner and to the same extent as the original kathode rays, this would be an argument in favor of Goldstein's original view. On the other hand, if the magnetic deflection should turn out to be distinctly different in amount, it would seem probable that the rays were not due to ordinary reflection, but to some other more complicated cause.

A Crookes' tube was prepared in such a way that the direct kathode rays and the reflected rays could be observed at the same time. This was accomplished by allowing the beam of rays from a concave kathode to fall upon a piece of sheet platinum placed obliquely at the focus. A small hole from this platinum target permitted a portion of the direct rays to pass on, while the reflected rays were sent off at right angles into a side tube. Screens of aluminum were arranged so that a sharply defined pencil of rays was obtained in each case. The rays were deflected by means of a permanent horseshoe magnet. The tube being in connection with the pump so that its vacuum would be fairly well maintained, alternate observations were made of the deflection of the direct and reflected beams. As the vacuum gradually changed, the magnetic deflection also varied, but no difference could be detected in the deflection of the direct and reflected beams, and if any difference exist it is much less than the difference produced in the direct rays by ordinary small changes in pressure.

As far as these experiments go they indicate that the para-kathodic rays differ in no respect from the direct kathode rays developed in the same tube. It therefore seems reasonable to assume that they are due to reflection, and assuming the truth of the modified Crookes' theory of kathode rays, it appears that negatively charged particles from the kathode stream rebound when they strike an obstacle with practically undiminished velocity.

[To be published in the Physical Review.]

ON THE ELECTRICAL PROPERTIES OF THE VAPORS FROM THE ARC. By Prof. ERNEST MERRITT and O. M. STEWART, Ithaca, N. Y.

THE vapors from an ordinary carbon arc are found to possess the power of discharging an electrical body, and in a general way resemble in this respect the vapors from a flame or gases which have been acted upon by X-rays. The vapors may be drawn through a tube of glass or metal to a distance of a number of feet from the arc and still retain their conducting power. Positive and negative charges are in most cases dissipated by the vapors with equal rapidity.

When the vapors are passed between a charged body and one that is grounded, the current flowing across is found to be nearly proportional to the potential difference so long as this difference is small; but for higher potentials the current increases less rapidly than the difference of potential and seems to approach a limiting value. The passage of a current through the vapors in large part destroys their conducting power.

The discharging power of the vapors may be greatly increased by bringing moisture into the neighborhood of the arc. It appears that the vapor of water is ionized under the influence of the arc to a much higher degree than are the other atmospheric gases; but the conducting condition does

not last so long in the case of water or vapor as in the case of ordinary air.

In general the behavior of the vapors can be explained by assuming that equal numbers of positive and negative ions are formed by the action of the arc. In some cases, however, indications of inequality were found. When a vertical enclosed arc was used and the vapors were drawn from a point close to the lower carbon, it was found that one charge was always dissipated by the vapors much more rapidly than the other; for example, if the lower carbon was positive, vapors taken from this neighborhood discharge negative electricity more rapidly than positive.

Further experiments upon this subject are in progress.

[To be published in the Physical Review.]

THE HEAT OF FUSION OF ICE DETERMINED IN ELECTRICAL UNITS. By Prof. E. L. NICHOLS, Ithaca, N. Y.

THE HYSTERESIS OF IRON AND STEEL AT ORDINARY TEMPERATURES AND AT THE TEMPERATURE OF SOLID CARBON DIOXIDE. By A. M. THIESSEN, Ithaca, N. Y.

DETERMINATIONS of the hysteresis losses in iron have recently shown that these losses become less for a given maximum induction as the temperature is raised. Fleming and Dewar have found however that, at the temperature of liquid air, the hysteresis loss in soft Swedish iron is practically the same as at ordinary temperatures. It would thus appear that the law of variation with temperature is different in the range between 0 and —200° from what it is at higher temperatures. The experiments here described were made to determine the hysteresis at a temperature intermediate between the lowest used by Fleming and Dewar and the ordinary temperature. It was intended to devote special attention to the nickel steel alloys, which are known to possess striking peculiarities in their magnetic behavior at low temperatures, but the samples of nickel steel were received so late that only a few determinations could be made with this substance, and no indication of abnormal behavior was found.

The specimens tested were in the form of rings turned out of the solid metal. The outside diameter was about 5cm., and the cross section was usually about 1 sq. cm.; in some cases a square or circular cross section was used, while in others the cross section was oblong, so that the inner and outer diameters of the ring were nearly equal. The latter form ensures greater uniformity in the value of H.

Observations were made at the temperature of boiling water, at ordinary temperatures, and at a temperature of about —70°, the latter temperature being obtained by immersing the ring and magnetizing coil in a bath of

ether containing dissolved CO_2. The coil and ring were protected from the ether by a layer of gutta percha. The magnetic measurements were made by the ballistic method, complete hysteresis loops being determined. The temperature was measured by means of a small coil of copper wire wound on the ring, the temperature coefficient of the copper having been previously determined.

The specimens tested were a ring of soft wrought iron, one of tool steel, and one of nickel steel; the latter contained 5.5 per cent of nickel.

Some of the results are contained in the following tables:

CRESCENT TOOL STEEL.

H	Temperature	Max. B	Hysteresis loss ergs per cc.
57.9	99°C	14300	28440
	17	14700	31380
	—52	14350	32580
4.42	98.5	2940	1650
	17.8	2480	1375
	—55	1670	698

NICKEL STEEL 5½%

H	Temperature	Max. B	Hysteresis loss ergs per cc.
52.8	99.5°	14160	36590
	28.5	14900	41870
	—65	15180	44400

SOFT WROUGHT IRON (ANNEALED).

H	Temperature	Max. B	Hysteresis loss ergs per cc.
11.9	95°	9930	4015
	20	9950	4590
	—63	10200	5102
2.6	97	3430	1710
	22	4985	1620
	—80	4670	1333
1.3	97	2520	433
	21	1320	371
	—78	1430	286

An inspection of the tables shows that for small magnetizing forces the hysteresis loss is greatest at the high temperature and least at the low temperature; while for large magnetizing forces the reverse is the case. This is true for each of the specimens. It will be observed, however,

that although the magnetizing force was made the same at each of the three temperatures used, the maximum induction was different, owing to the fact that the permeability changes with the temperature. By assuming the truth of Steinmetz's law it was possible to interpolate so as to determine at each temperature the hysteresis loss corresponding to a given induction. When this was done it was found that the hysteresis loss at high temperatures was always less than at lower temperatures. In the case of the tool steel, for example, for an induction of 14700 the hysteresis loss at 99° was 29600, while at a temperature of —52° and the same maximum induction the loss was 33850.

The results have not yet been fully worked up.

THE ELECTRICAL RESISTANCE OF LEAD AMALGAMS AT LOW TEMPERATURES.

By G. W. GRESSMAN, Ithaca, N. Y.

THE object of the experiments here described was to determine the temperature coëfficient for resistance in the neighborhood of the freezing point of the amalgam tested. Amalgams were prepared containing various proportions of lead, ranging from pure mercury to 25% lead. The specimens were contained in little U-tubes of small bore (about 1 to 2 mm.), and were cooled to a temperature below their freezing points by a bath of ether and carbonic acid snow. The resistance was measured by the fall of potential method both while the amalgam was cooling and while it was warming up again to the temperature of the room. The temperature was determined by a small coil of copper wire wrapped closely around the U-tube from end to end.

As the results have not yet been fully computed only a general statement can be made concerning them. The freezing point of the amalgam was very sharply indicated by a sudden change in the resistance, the resistance of the solid amalgam being sometimes only one-fifth as great as the resistance of the liquid. The freezing point as determined by this sudden resistance change was found to depend upon the amount of lead dissolved. All the amalgams tested melted at a higher temperature than the pure mercury. The temperature coëfficient of the amalgam between ordinary temperatures and the freezing point was found to be nearly constant As the amount of lead dissolved was increased, the temperature coëfficient diminished, and in the case of the 25% amalgam was extremely small. This amalgam, which is only semi-liquid, shows a strange break in its resistance curve in the neighborhood of 30°, the coëfficient being apparently positive for temperatures below this point and negative for higher temperatures. This concentrated amalgam is, however, rarely homogeneous, and seems to consist of at least two parts which differ greatly in their properties.

THE MOST EFFICIENT THICKNESS OF TRANSFORMER PLATE. By Prof. F. BEDELL, R. M. KLEIN and T. P. THOMPSON, Ithaca, N. Y.

IN a laminated magnetic circuit the loss due to hysteresis is

$$V n \eta B^{1.6} 10^{-7} ;$$

that due to eddy currents is

$$V \gamma (t n B)^2 10^{-21}$$

where V is volume in cu. cms., n is frequency, B is magnetic density, t is thickness of plate in mils., η is hysteresis coëfficient, and γ is an eddy current coëfficient. There is a definite value to the thickness t which makes the total loss a minimum under particular conditions.

(To be published in the Physical Review, Sept. 1898.)

THE RESISTANCE OF IRON WIRES FOR ALTERNATING CURRENTS. By Prof. ERNEST MERRITT, Ithaca, N. Y.

THE resistance of iron wires to alternating currents of a given frequency cannot be computed with accuracy on account of the uncertainty in the proper value to be given to μ. If we give to μ a value of about one thousand, which seems a reasonable one for wrought iron, the ordinary method of computation would indicate an enormous resistance even at very low frequencies. But it is evident that the resistance must depend upon the current flowing in the wire, for when the current is large the outer layers of iron will be saturated and the permeability will in consequence be small. Since the permeability will be different at different depths below the surface, and will also vary during the course of an alternation, it seems questionable whether the ordinary method of computing the resistance will give results which are even approximately correct. The following experiments were undertaken at the suggestion of the writer by Mr. H. H. Denio, in order to throw some light on the question as to how accurately the resistance could be predicted.

The resistance of the iron wire was determined by a bridge method in which a telephone was used in place of a galvanometer. One arm of the bridge was made of a short piece of the wire in question, while the other three arms were of German silver. The wires were stretched upon a board frame at a considerable distance from each other so that errors due to self-induction and mutual induction should be as small as possible. It was impossible to obtain an adjustment which gave absolute silence in the telephone, but the position of minimum sound could be located with considerable accuracy. With each wire experimented upon, determinations were made for a variety of different values of the current flowing

in the iron. The resistance of the wire was measured by the same apparatus for direct currents of equal strength. Curves were then plotted showing the relation between the current flowing in the wire and the ratio of the alternating current resistance to the ordinary resistance. It was necessary to use this ratio in plotting in order to eliminate the resistance variation due to temperature changes.

A great variety of iron and steel wires was tested, but the results were the same in character in all cases, even when the diameter of the wire was changed through a wide range. For low currents the resistance was practically the same for direct and alternating currents. As the current strength increased the resistance offered to alternating currents became greater, and finally reached a maximum. It then decreased again as the current strength was made still greater, and with a sufficiently strong current the resistance was again practically the same for direct and alternating currents. The maximum value of the resistance was about three times that offered to direct currents.

In a general way these results are explained by changes in the saturation of the iron resulting from changes in the current flowing. For very small currents the permeability is known to be low, so that an iron wire would behave in much the same way as a copper wire. For larger currents, the iron being brought into a condition of greater permeability, the throttling effect is greater; but when the current has been increased to such an extent that the outer layers of the wire begin to saturate, there is no longer any tendency for the current to concentrate at the surface, and the resistance again approaches the ordinary value.

[To be published in the Physical Review.]

ON THE EFFICIENCY OF CONDENSERS. By Prof. E. B. ROSA and ARTHUR W. SMITH, Middletown, Conn.

THE enormous discrepancies in the results of various observers as to the amount of heating in condensers, and the almost entire lack of precise statement as to its numerical value, led us to measure this energy loss in such a way that it could be expressed absolutely. The preliminary report of our work was presented at the Detroit meeting last year, and the method is here briefly described.

THE RESONANCE METHOD.

A low voltage alternating E. M. F. is impressed upon a condenser and coil in series. The relation between the capacity C and self-induction L being $C = \frac{1}{p^2 L} \cdot p = 2\pi n$. The resulting resonance produces a high voltage at the terminals of the condenser. The current in the lead wires is in phase with the impressed E. M. F. and a wattmeter will accurately meas-

ure the energy used by both condenser and coil. Subtracting the rI^2 loss in the coil leaves the amount of energy dissipated in the condenser.

Another arrangement was to place the coil and condenser in parallel and impress a high E. M. F. upon both. Each takes its own current, but the two currents being so nearly opposite in phase, the resultant current in the lead wires is very small and in phase with the E. M. F. Hence the power can be measured by a wattmeter as before.

THE EFFICIENCY OF A CONDENSER.

Having thus determined the energy dissipated in a condenser, it remains to discuss the efficiency. For simplicity, consider both current and E.M.F. to be simply harmonic. If the current is 90° ahead in phase, as would be the case in a perfect condenser, the power curve will consist of equal positive and negative loops. As the angle of advance decreases the positive loops will increase and the negative loops decrease, the difference between the enclosed areas being a measure of the energy dissipated. Thus the area of the positive loop represents the work done upon the condenser in charging it, and the area of the negative loop represents the work done by the condenser in its discharge.

The efficiency is the ratio of the work done by the condenser in its discharge to the work done upon the condenser in charging it. This is the *gross efficiency*.

SECOND DEFINITION OF EFFICIENCY.

Regarding the condenser as an instrument for storing electrical energy, and in which a certain amount of energy is dissipated in the process, we may define the percentage of loss as the ratio of the energy dissipated to the energy stored. The efficiency is then unity minus the loss. This we may call the *net efficiency*. Then,

$$\text{Net Efficiency} = 1 - \frac{\text{Energy Dissipated}}{\text{Energy Stored.}}$$

A CALORIMETRIC DETERMINATION OF THE ENERGY DISSIPATED IN CONDENSERS. By Prof. E. B. Rosa and Arthur W. Smith. Middletown, Conn.

The condenser is placed within a calorimeter of special construction and subjected to an alternating E. M. F. After a few hours the heat given off by the condenser reaches a steady value and can be measured. Described briefly, the calorimeter is a box large enough to contain the condenser, and surrounded by three concentric walls of wood. By special arrangements there is no gain or loss of heat through the walls. The heat generated by the condenser is absorbed and carried away by a stream of cold water flowing through copper pipes. The rate of absorption is regulated so as to maintain the temperature of the calorimeter constant.

The accuracy of the calorimeter was tested by measuring the heat generated by an electric current through a resistance. The heat measured was equivalent to 12.37 watts, and the electrical measurements gave 12.34 watts.

A good many determinations have been made with different condensers and with the same condenser under varying conditions of temperature and voltage. In general, we have found about 1% loss in paraffin condensers and five times as much in beeswax and rosin condensers.

ON THE GRAPHICAL TREATMENT OF MUTUALLY INDUCTIVE CIRCUITS, WITH SPECIAL REFERENCE TO THE CASE OF VARIABLE FREQUENCY. By Dr. HENRY T. EDDY, Minneapolis, Minn.

THIS paper treats first the air transformer with a given alternating current in the primary circuit, finding analytically the nature of the locus as the frequency varies of the extremity of the vector representing the primary E. M. F., and showing how it may be readily constructed by help of considerations peculiar to such loci.

The case of a given alternating E. M. F. is next considered, and the locus of the extremity of the vector representing the primary current is constructed.

These constructions are exhibited for two mutually inductive circuits with assumed numerical values of self induction, mutual induction and resistance, by taking first one circuit as the primary and then the other.

Thirdly, it is shown that the locus of the extremity of the vector representing the secondary current is a circle, and it is the same circle whichever circuit be taken as the primary in which the given E. M. F. acts.

A similar statement as to the case of a given current in the primary makes the locus representing the secondary E. M. F. a circle, and it is the same circle whichever circuit is the primary.

These circles furnish an alternative method of constructing the loci of the primary.

Lastly, the paper treats the combined currents in the two circuits in case the given E.M.F. act upon them simultaneously connected in parallel, this being a composition and combination of the separate results previously obtained to give loci representing the total current in each branch and both branches of a split circuit in case of mutual induction between the branches.

[This paper is published in the Transactions of the American Institute of Electrical Engineers.]

AN APPARATUS FOR DETERMINING COEFFICENTS OF INDUCTION. By Prof. SIDNEY T. MORELAND, McDonogh, Md.

Two rectangular coils of about the same size except that one is small enough to be placed inside the other are arranged with their planes at right angles to each other so as to enclose a common space $1\frac{1}{2}'' \times 1\frac{1}{2}'' \times 3\frac{1}{4}''$. In this space is delicately suspended a hollow copper cylinder as

large as it may be to turn freely. A mirror is attached to the support of the cylinder and by means of a lamp and scale any slight motion of the cylinder may be detected. If each of the coils is placed in one branch of a divided circuit carrying an alternating current in general a rotary field will be produced in the space common to the two coils and the suspending cylinder will turn until the resisting torque of the suspending wire balances the torque produced by the rotary field. Let L_1 and r_1 be the coëfficient of self induction and the total resistance in one branch of the divided circuit and L_2 and r_2 similar quantities for the other branch. Then if r_1 be so adjusted that $\frac{L_1}{r_1}=\frac{L_2}{r_2}$ no rotary field will be produced and the cylinder will not be deflected. If another coil L_3 and r_3 (r_3 including any non-inductive resistance that may be added) be put in series with L_1 r_1, then for no rotation we must have $\frac{L_1+L_3}{r_1+r_3}=\frac{L_2}{r_2}$, but this can only be true when $\frac{L_1}{r_1}=\frac{L_3}{r_3}$ or $L_3=\frac{r_3}{r_1}L_1$. From which it is seen that if L_1 has been previously determined by some independent method, L_3 may be determined by measuring r_3 and r_1; we may measure r_1 once for all and hence we have only to adjust r_3 so as to get no deflection and then measure r_3 in order to determine L_3.

AN APPARATUS FOR DEMONSTRATING, IN ALTERNATING CURRENTS, THE CHANGE OF PHASE DUE TO EITHER INDUCTANCE OR CAPACITY. By Prof. SIDNEY T. MORELAND, McDonogh, Md.

THE apparatus consists of a circuit divided into two branches in each of which is a conductor stretched across the field of a strong electromagnet and supplied with arrangements for delicate adjustment of the tension. When properly tuned the conductors vibrate under the influence of the alternating current. Each of these conductors has a small mirror at one end of it; the conductors are placed at right angles to each other with the mirrors *vis à vis* and so adjusted that a beam of light may be successively reflected from them. This arrangement gives, of course, Lissajou's figures when the conductors are vibrating; if the phase is the same in the two branches the resultant is a straight line; if there is a difference of phase this resultant will in general be an ellipse. To show the direction of motion in describing the ellipse, in order to determine which conductor is behind the other in phase, the light is made intermittent by means of a tuning fork. If the two branches of the divided circuit are alike otherwise but an electro-magnet is placed in one branch, the ellipse will appear with the motion in a certain direction around it; if now capacity be substituted for the electro-magnet the direction of motion is observed to reverse.

ON THE VELOCITY OF LIGHT IN A MAGNETIC FIELD: A REPORT OF EXPERIMENTS MADE WITH THE AID OF A GRANT FROM THE RESEARCH FUND OF THE ASSOCIATION. By Prof. EDWARD W. MORLEY, Cleveland, Ohio; Prof. HENRY T. EDDY, Minneapolis, Minn.; and Prof. DAYTON C. MILLER, Cleveland, Ohio.

PART III.[1] RESULTS OF THE EXPERIMENT.

1. With the apparatus described in the first part[2] of this report, we used a current which produced a rotation of fifty degrees in the plane of polarization of the light emitted by a Bunsen flame, fed with sodium carbonate, the light passing twice through the bisulphide.

When half of a ray of light suffers such a rotation before producing interference with the unchanged half, the intensity of the minima is of course lessened. This lessening was, in our experiment, by no means of such a character as obviously to interfere with the accuracy of observation.

One observer produced the desired magnetic field alternately in the two coils announcing the alternations by using signal words which kept the other observer ignorant of the direction of the current. The second observer, selecting a time when the unstable interference fringes assumed a sensitive form, endeavored to detect any displacement due to the current, and to determine whether its direction indicated retardation or acceleration of the light. Both observers (Eddy and Morley) were inclined to believe that there was a displacement, amounting to perhaps a fiftieth of a wave length; by suitable very slight rotation of the plate $c\ d$,[3] each observer decided whether the displacement indicated acceleration or retardation. It may be said that all the observations agreed as to the sign of the effect indicating an acceleration. If the observers could feel confidence in this result, we should have to infer an acceleration of about one part in a hundred million in a column of bisulphide, sixty centimetres in length surrounded by a magnetic field producing a rotation of fifty degrees in the plane of polarization of light corresponding to the D line of the solar spectrum. But the interferences seen were never stable enough to admit of certainty as to one-fiftieth of a wave length.

2. When this experiment was reported to the Association in 1890, Section B recommended a further grant in order to repeat the experiment with a greater rotation. At first it was proposed to obtain a rotation of a whole circumference; but the behavior of carbon bisulphide in so long a column was uncertain. It was therefore thought proper first to obtain a rotation of *half* a circumference; and this has now been secured. Since the result is negative, not many details as to dimensions of parts of the

[1] Parts I and II of this report may be found in Proceedings American Association, 1890, p. 81.

[2] Proceedings American Association, 1890, p. 82.

[3] Figure on page 81, Proceedings, 1890.

apparatus need be given, except to say that the lengths of coil, and of column of bisulphide, were twice those used before, and that a current of twenty-seven amperes produced rotation of half a circumference of the plane of polarization of light corresponding to the *D* line, when the light passed through one hundred twenty centimetres of bisulphide.

We also used a more elaborate method of securing the thermal and optical stability of the carbon bisulphide, as was made very necessary by the double length of the apparatus. We had a circulation of water and a circulation of bisulphide; and the tank which supplied the current of bisulphide was immersed in that which supplied water. If any further step is to be made, it will be necessary to work with a current of bisulphide of five, ten, or twenty feet a second.

3. A current was sent through the coils at the call of the observer at the telescope. This caused the interference bands to shift by one-half wave length, due to the reversal of phase of one ray. A micrometer wire was set on a maximum or minimum, the current was reversed, and the coincidence of the maximum or minimum with the wire was scrutinized. As a result of such scrutiny, both observers (Morley and Miller) agree in affirming that there was no such displacement in their experiments as one-twentieth part of a wave length and that there is no advantage to be expected from increasing the size of the apparatus and repeating the experiment with carbon bisulphide, unless it shall be kept optically homogeneous by rapid motion. If dense glass, sufficiently transparent in long columns, could be used, it would probably be possible to detect a quantity which, if a liquid is used, could not be detected except at great expense.

We have then to report, that we are confident that when light corresponding to the solar *D* line passes through one hundred twenty centimetres of carbon bisulphide in a magnetic field sufficient to produce rotation of the plane of polarization by half a circumference, there is no such change of velocity as one part in sixty million; probably not one part in one hundred million.

ON THE OSMOTIC PRESSURE OF CERTAIN ETHER SOLUTIONS AND ITS RELATION TO BOYLE-VAN'T HOFF'S LAW. By H. M. GOODWIN and GEORGE K. BURGESS, Boston, Mass.

THE authors have determined the osmotic pressure of ether solutions of naphthalene, benzophenone and diphenylamine at 12.9° C. from measurement of the vapor pressures of these solutions. The concentrations of the solutions varied from about one to twenty per cent by weight. The vapor pressure of each solution was measured directly by causing the solution to boil under such a diminished pressure that the temperature was that of the boiling solvent. The temperature was indicated by a platinum resistance thermometer placed in the boiling liquid. This ther-

mometer formed one arm of a Wheatstone bridge, the three others of which were manganene coils, maintained at uniform temperature in a thermostat. A galvanometer capable of detecting a change of temperature of 0.001° of the platinum coil was employed. 12.90° was chosen as temperature to permit the use of Ramsay and Young's data on the specific volume of ether vapor, in the computation of the osmotic pressure, from the vapor pressure measurements.

Specific gravity determinations of the solutions were also made at 12.90° C. to permit of the reduction of concentrations to molecules of solute per litre of solution. The reciprocal of this, *i. e.*, the volume V, occupied by one molecular weight of dissolved substance, was then compared with the corresponding value of the osmotic pressure P. Plots were made with values of PV as ordinates and P as abscissæ. It was found that this product was a constant, for none of the three solutions investigated, as it should be if the laws of a perfect gas were exactly followed. In all cases the product diminished with increasing concentration, but by different amounts, the curves resembling closely in form those representing similar relations for the more easily compressible gases. In the case of diphenylamine the product PV approached a constant or possibly minimum value at pressure of about sixteen atmospheres. Benzophenone showed no tendency to approach a minimum at even greater pressures than this; while naphthalene occupied an intermediate position. As the probability of dissociation phenomena in these solutions is exceedingly small, it would seem that each of the substances investigated has its own peculiar condition equation $\varphi\ (t\ p.\ v)=0$ just as is the case with gases and vapors, with the exception of the few so-called permanent gases. Azobenzine in ether solutions (investigated by Noyes and Abbot), seems on the other had to follow very closely Boyle-Van't Hoff's law.

ON THE DIELECTRIC CONSTANT AND ELECTRICAL CONDUCTIVITY OF LIQUID AMMONIA. By H. M. GOODWIN and M. DE KAY THOMPSON, JR., Boston, Mass.

It has been pointed out by J. J. Thompson and Nernst that the dissociating power of a solvent is intimately related to its dielectric constant (D. C.). So far as a comparison of these two properties can be made from existing data an approximate proportionality has been found to hold between them. Thus water of all solvents forms by far the best conducting solutions and has the abnormally high dielectric constant of about eighty. It might be expected therefore that if a solvent were found with a still greater D. C. its power of forming electrolytic solutions would be greater than that of water, and conversely. No liquid with a greater D. C. than that of water has been discovered, but it has been recently shown by Cady that the molecular conductivity of many salts dissolved in liquid ammonia is greater than the corresponding molecular conductivity which they possess in water. Liquid ammonia would seem from this to possess a dissociating power greater than water. It was

thought therefore to be of considerable interest to measure its D. C. in order to determine if the Thompson-Nernst relation also held in this case.

The method employed was that described by Drude. It consisted essentially in generating a series of short electrical waves in a system of parallel wires, bridging the wires across with a cell containing electrodes immersed in the liquid under investigation and noting the position of the cell on a graduated scale when the length of the secondary circuit had been adjusted to maximum resonance. This was determined by noting when a vacuum tube placed in the path of the waves lighted up with its maximum brilliancy. The apparatus was calibrated by noting the position of maximum resonance with the cell filled with benzene, acetone, water, and mixtures of these liquids, the D. C. of which was known.

The following results were found for various preparations of liquid ammonia, at — 34° C. ± 1°.

Commercial liquid ammonia	21
Prepared from lime and ammonium chloride . .	23
Distilled from commercial ammonia over lime . .	22
Ammonia over lime	21.5

These values instead of approximating the value for water are, it is seen, of the order of magnitude of the D. C. of acetone and of alcohol both weakly dissociating liquids. Were the comparison made at a higher temperature, the above values would undoubtedly be still smaller as Abegg has shown that the D. C. increases in general rapidly with falling temperature. It would seem, therefore, that liquid ammonia forms a marked exception to the Thompson-Nernst relation. In view of this fact, it seemed desirable to confirm certain results on the abnormally high conducting power of ammonia solutions.

For this purpose a method was devised by which the ammonia could be distilled directly over into the conductivity cell and measured under pressure, thus avoiding evaporation and boiling at the electrodes during the measurements and permitting measurements to be made at any temperature by immersing the cell in suitable baths.

Pure liquid ammonia was found to have a specific conductivity of 1.6×10^{-5} at —15° to —20° and a temperature coëfficient of 1.6% per degree. Solutions of silver nitrate in liquid ammonia were measured and the following values found for the molecular conductivity:

Volume in litres. v	$u \times 10^{-7}$	Temp.
110	180	—16.4°
94	176	—16.3
78.9	171	—16.2
64.0	163	—15.2
54	153	—14.5
30	134	—12.2
140	147	—34 (Cady)

The values increase more slowly as the dilution increases as in the case of aqueous solutions; but are, even at the low temperature at which they are determined, considerably higher than the corresponding values of aqueous solutions. It would seem, therefore, that in liquid ammonia solutions either the velocity of migration of the ions is enormously greater than in water, or, and this seems more probable, the dissociation itself is in reality large, in which case the Thompson-Nernst law suffers an exception.

PROGRESS IN THE EXPLORATION OF THE AIR WITH KITES, AT BLUE HILL OBSERVATORY, MILTON, MASSACHUSETTS. By A. LAWRENCE ROTCH, Director.

IN August, 1894, a meteorological instrument, recording graphically and continuously, was lifted by kites for the first time and this method of obtaining data in the free air with improved apparatus has been in use since at Blue Hill. The apparatus employed until February, 1897, has been described by my assistant, Mr. Fergusson, and the data obtained have been discussed by another assistant, Mr. Clayton, in Annals of the Astr. Observatory of Harvard College, Vol. XLII, Part 1, Appendix B. Subsequently by means of new apparatus constructed with the aid of a grant from the Hodgkins Fund of the Smithsonian Institution, much greater heights were reached. For example, in October, 1897, records of the pressure of the atmosphere, with the temperature and relative humidity of the air, were brought down from more than eleven thousand feet above Blue Hill and the average height attained by the meteorograph in all the recent flights is a mile and a half. In consequence of reports made by the writer to the International Meteorological Conference at Paris in 1896 and to his colleagues of the International Aeronautical Committee at a meeting last April, kites are now being used in Europe to obtain meteorological data and the value of the method is generally recognized.

[This paper will be printed in the Monthly Weather Review of the U. S. Weather Bureau.]

TEMPERATURE AND VAPOR GRADIENTS IN THE ATMOSPHERE. By Prof. FRANK H. BIGELOW, U. S. Weather Bureau, Washington, D. C.

(Read in Joint-Session with Section A.)

THE thermodynamic conditions of the atmosphere have long been studied by meteorologists, but the recent development of upper air observations by means of clouds, balloon and kite ascensions, have rendered the subject of even more importance. I have, therefore, made a comparative examination of the work of the leading authorities, and present a brief account of my conclusions. There are two types of formulæ, namely, the exponential and the logarithmic, upon which the entire subject

rests. It may be shown that these are drawn from the same fundamental thermodynamic equations.

After adopting a certain system of notation it can be proved that the sets of formulæ used by Ferrel, Hann, Sprung and Sohncke, are exponential and identical; also that the formulæ employed by Hertz and von Bezold are logarithmic, and when fully expanded are in agreement. Furthermore, the set of exponential formulæ can be transposed rigorously into the logarithmic set, so that in fact this great body of material presents only different aspects of the same fundamental processes.

These formulæ are adapted to study the five stages or conditions existing in the free atmosphere: the dry, the unsaturated (dry air + aqueous vapor), the saturated (dry air + vapor + water), the freezing (vapor + water + ice), the frozen (dry air + vapor + ice). The four last mentioned are the Hertz α β γ δ stages, respectively. Now when we come to examine the practical use of these theoretical conclusions, resting as they do upon firm foundations, we find that little has ever been done with them. The observations themselves show that the atmospheric gradients of temperature and vapor contents do not conform very closely to the adiabatic limitations involved in the formulæ. On the other hand, no systematic study has been made of the exact physical changes required to pass from the adiabatic to the natural gradients which are actually observed. It is my purpose to make such a study for the work of the Weather Bureau.

Hertz made up a diagram of the curves involved in his formulæ for practical purposes, but in constructing them several of the terms in the formulæ were deprived of the vapor contents entirely, so that his curves are only approximations, and the process not rigorous or suitable for cloud studies, wherein even minute quantities of vapor must be retained. If his rigorous formulæ are computed for each case, the process is so tedious that it will not be practically employed by any one. I have, therefore, attempted to secure a rigorous method of utilizing these valuable processes. To begin with, it can be shown that the entire system of formulæ has functions of only three quantities p, t, e, pressure, temperature and vapor tension. One specimen of the three types of formulæ, from the saturated (ξ) stage will be written down for comparison.

Exponential Formula. (ξ).

$$\left[A\left(1-.378\frac{e}{p}\right)RT+r\mu\right]\frac{dp}{p}=\left[c_p\left(1-.378\frac{e}{p}\right)+\frac{r\mu}{T}\right]dT.$$

Logarithmic Formula. (ξ).

$$0=(c_p+\ldots)\log\frac{T}{T_s}-2.4R\log\frac{p-e}{p_s-e_s}$$
$$+\frac{2}{R}\left[\frac{r}{T}\frac{e}{p-e}-\frac{r_s}{T_s}\frac{e_s}{p_s-e_s}\right]$$

Bigelow's Formula. (β).

$$\left[.2374 + .4748\,\frac{e}{B} + .145\,\frac{e^2}{B^2}\right]\log T - \left(.06858 - .04266\,\frac{e}{B}\right)\log\,(B - e') + \left[\frac{497.5}{T} - .4404 - \left(\frac{309.4}{T} - .2739\right)\frac{e}{B}\right]\frac{e'}{(B-e')} = C_\beta$$

Similar formulæ are developed for all the other stages.

Now these rather complex formulæ can be reduced to double entry tables, which are very easy to use not only with rigor but also rapidly. A complete example was shown of the method of tracing the variations in a specimen kilogram of the atmosphere, on moving it from the ground upwards, while it passes through all the stages. Comparison was made with the results obtained with the logarithmic formulæ, and with the approximate values found by the Hertz diagram. Some account was also given of the practical uses of this method in cases of inversion of temperature, in the vertical gradients, and in other cases coming under observation.

THE STRUCTURE OF CYCLONES AND ANTICYCLONES. By Prof. FRANK H. BIGELOW, U. S. Weather Bureau, Washington, D. C.

(Read in Joint-Session with Section A.)

AT the Detroit meeting of the Association, 1897, some account was given of the mathematical status of the theories which have been advanced by Ferrel, Oberbeck, Pockels, Sprung and others, to account for the movements of the air in cyclones and anticyclones. It was shown in general that these solutions depend upon certain assumptions, which make the circulation *ideal* rather than natural, and that there are several grave difficulties in the way of accepting the theories involved as competent to give a complete solution of the problem. A brief review of the various proposed forms of vortex motion was made by the speaker.

As a preliminary to the true theoretical solution of this complex problem of fluid motion, it is necessary to secure a correct knowledge of what the stream lines of motion in the atmosphere really are, which produce cyclonic and anticyclonic effects at the surface of the earth. It was for this purpose that the International Cloud Observations were carried on during the years 1896, 1897, and by means of them the United States Weather Bureau has obtained more than 30,000 observations, available for the purposes of this discussion. They were made with the same type of nephoscope at fourteen stations, the Blue Hill Observatory also coöperating; the reductions have been executed on a comprehensive plan, and the results are definite and satisfactory. If these observations could

be continued for two or three years longer, certain imperfections and irregularities in the final vector systems would doubtless be entirely eliminated.

The first general result was the determination of the mean velocities in the eight cloud strata over the high areas and the low areas. A diagram was exhibited and explained, showing that the average velocity is as follows, in meters per second and in miles per hour.

CLOUDS.	HIGH AREAS.		LOW AREAS.	
Cirrus Cirro-stratus	35	78	40	90
Cirro-cumulus	34	76	39	87
Alto-stratus	29	65	35	78
Alto-cumulus	23	52	29	65
Strato-cumulus	23	52	29	65
Cumulus	11	25	15	34
Stratus	11	25	11	25
Wind	5	11	5	11

A very interesting fact appears, namely, that the atmosphere apparently is traversed by three distinct currents: the lower in the stratum 900 to 1,500 metres high, the middle at the stratum 2,500 to 4,500 metres, and the upper at the stratum 6,500 to 8,000 metres. Furthermore at the bottom of each of these layers, which move with nearly uniform velocity throughout their entire depth, it is found that the stratus forms of clouds occur, and that at the top of each of these streams the cumulus types are observed. This suggests that in every case stratus clouds develop at the bottom of a moving stream of air and cumulus clouds at the top, the former depending upon the difference of velocity and temperature, which always marks the boundary of two layers of air moving over each other, and the latter upon the convectional ascension within the layer of air whose velocity is approximately uniform throughout its depth. This comprehensive classification of cloud forms receives much support in the physical facts that are known to accompany cloud formations.

The next result described was the chart of the mean annual vectors of the air movement in the several cloud layers over the high and the low areas. By placing a low area between two adjacent highs, to show the continuous motion through a complete system of circulation, the following features are displayed: in the cirrus (9,800 m.) the vectors are nearly all eastward with a slight sinuous wave above the highs and below the lows; in the cirro-stratus (9,800 m.) this is a little more pronounced; in the cirro-cumulus (8,100 m.) the wave is clearly marked; in the alto-stratus (5,900 m.) the wave is somewhat deeper; in the alto-cumulus (4,500 m.) the gyratory components are evidently stronger, but there is only a deep sinuous stream line; in the strato-cumulus (2,500 m.) some of the southern vectors of the high begin to turn westward, and the northern vectors of the low are thrown upward; in the cumulus (1,500 m.) is first formed a complete anticyclonic circulation, well developed in all the quadrants,

but the cyclone is only partially rotated; in the stratus (900 m.) and at the ground, the well-known directions of movement are found fully formed. These vectors are compounded of the eastward drift and the rotary components.

A diagram was next shown giving the purely gyratory components, after eliminating the eastward drift. Omitting the details, it shows that the anticyclonic components extend from the cirrus (9,800 m.) to the ground; in the case of the lows the cyclonic components disappear at the cirro-cumulus level (8,100 m.), about one mile below the elevation to which the anticyclonic components were traced. In Professor Ferrel's theory, the cyclone extended upward by convectional action above, and overflowed upon the high area. The evidence is here precisely opposite in its tendency, the anticyclone being found at greater elevations than the low area, and hence the Ferrel theory is not sustained.

In order to condense the data and eliminate to some extent the minor irregularities, the nine sets of vectors were reduced to three by uniting respectively, the ci., ci.s., ci.cu.; a.s., a.cu., s.cu.; cu, S. wind. The conclusion is most instructive. There are three sets of vectors at the mean distances 250, 750, 1250 kilometers from the centre or 155, 466, 777 miles, respectively. In the case of the high areas the vectors on the two outer circles all point outward from the centre; near the centre, in the upper levels the vectors point inward, in the middle levels they are tangential, and in the lower levels they point outward. Therefore, at the centre there exists a true vortex with reversal of gradient slope; on the two outer circles there is only the deflection due to two horizontal currents flowing together, or past each other, and thus sustaining outward gradients throughout the strata taken vertically. In the case of the low area the vectors are all directed inward from bottom to top on each of the circles, and there are no outward components such as Professor Ferrel supposed to exist in the upper strata. Hence the evidence is conclusive against the commonly accepted theory of convectional circulation in cyclones. To explain the nature of this kind of vortex motion would carry us beyond the scope of this paper, but it is evidently of that mathematical type of forced circulation which connects a magnetic whirl with the electric current through it, with continuous absorption of energy towards the centre. The preliminary application of this theory to the data in hand promises to give us an interesting and probably satisfactory theory of the subject. The diagrams show that the stratum of maximum free activity, that is aside from the eastward drift, is in the cumulus region; that the anticyclone precedes in size and energy the development of the cyclone; that horizontal currents, which seek to flow past each other, set up a true vortex at the centre; that this discharges down and draws in material from above, much as an electric fan sets in motion a long column of air, and thus causes the sinuous deflection in the upper strata; that two adjacent highs by their counterflow produce a cyclonic couple, rotate the air about a centre and force upwards a continuous column, to be discharged within the fast moving upper strata, and there to be aimlessly dissipated; that

latent heat or local overheating at the surface may assist this movement, but is not essential to it; that long horizontal counterflows, due primarily to interchange in the cumulus stratum between the warm air of the tropics and the cold air of the polar zones, are the primary forces concerned. The functional relation between vertical and horizontal components is that of the curl, like the cross connection between electric current and magnetic whirl.

A FUEL COMBUSTION BOMB. By C. L. NORTON, Boston, Mass.

THE paper describes a modified Berthelot bomb. It consists of two hemispherical shells screwed together by a nut. All parts of the apparatus are composed of aluminum bronze. No interior lining is used as the bronze is not acted upon by the oxygen at the temperature of the explosion. Particular attention is called to the method of constructing the valves and joints. It is hoped the bomb will prove suitable for purely technical as well as scientific uses.

ON THE USE OF WINDOW GLASS HAVING A DIFFUSIVE ACTION ON LIGHT. By C. L. NORTON, Boston, Mass.

THE action of a window glass having an irregular or uneven surface in diffusing light throughout a room has been the subject of a lengthy research. Comparison has been made between the intensity of light in the corresponding parts of two similar rooms, glazed with different kinds of window glass. It is found that the action of a ribbed or corrugated glass is to prevent the strong light from the sky from falling upon the floor near the window and to cause the light to be spread throughout the entire room.

A DEVICE FOR RECORDING PHOTOMETER SETTINGS. By CHARLES P. MATTHEWS, Lafayette, Ind.

IN the ordinary photometric process there occurs between successive settings a considerable interval, in which a reading of the bar is taken and recorded, either by the observer or an assistant. Various expedients for reading the bar are resorted to, as, for example, the use of a hand mirror to reflect a beam of light from one of the sources or the turning on of a glow lamp. These methods are not free from objection. The sudden influx of light produces in the observer fatigue of the retina and of the pupillary muscles and there is a consequent loss of visual sensitiveness. Furthermore, when it is a question of the photometry of a fluctuating source, the infrequency and irregularity of the readings are a

drawback. In arc-light photometry the illumination of the photometer disk is a shifting quantity. Some means of recording a setting as soon as made, thus allowing the observer to proceed to a subsequent setting without interruption or the necessity of removing his gaze from the photometer screen, would seem likely to yield more satisfactory results, with a considerable economy in time.

In the course of experiments on the alternating arc the writer has used a recording drum. A cylinder of wood 10 cm. in diameter and about 1.5 m. in length is mounted with its axis parallel to and somewhat below the top of the photometer bar. At the end of a spring attached to the photometer carriage a steel point projects downward nearly to the surface of the cylinder. A rod extends the length of the cylinder and carries at one end a pawl which engages the teeth of a ratchet wheel at the cylinder end. The act of depressing sharply the rod, advances the cylinder a certain angular amount and punctures the paper by means of the steel point.

For identifying the records two methods suggest themselves. The point may, at the close of the measurements, be placed over each puncture successively and the corresponding reading taken from the bar, or the paper covering the cylinder may be ruled and marked in the same manner as the bar itself, when the settings may be read directly from the paper. This latter has the advantage of making a permanent record. It is not necessary to have the cylinder of a length greater than $\frac{1}{2}$ to $\frac{3}{4}$ the length of the bar. When mounted on the bar in the manner shown it can be moved so as to accommodate a widely fluctuating series of readings. If it is desired to refer the readings to time, a battery circuit may be arranged so that contact between the rod and the spring will give a bell signal. Two additional observers are then necessary; one to call and one to record time. For a single observer to take readings referred to time the drum must be turned at a known rate by means of a clockwork. This is not a difficult matter to arrange. When it is desired to make a large number of settings without regard to their sequence the device may be used with the pawl lifted. If two impressions of the point are superimposed the appearance of the puncture usually reveals the fact.

The records obtained possess the natural advantages peculiar to graphical results. The apparatus lends itself admirably to the study of personal error as any peculiarities are at once manifest, while the observer's ignorance of his settings precludes bias. Some interesting personal records have been obtained and will be included in a later report.

NOTE ON EFFECT OF SILICON ON PERMEABILITY OF CAST IRON. By Prof. F. C. CALDWELL, Ohio State University, Columbus, Ohio.

THIS paper gives the results of tests made by J. M. Hipple and S. R. Hewitt under the direction of the writer. Eleven samples of Rockford and fourteen of Bessemer cast iron were tested in bars $\frac{3}{4} \times 6\frac{1}{2}$ inches,

using Ewing's double yoke method. The variation of silicon was obtained by the addition of silvery iron.

The results show that the reluctivity is decreased with the addition of silicon, following a straight line law between 1.8 and 4.6% for B = 2000 and B = 5000 and between 1.8 and 3.5% up to 8000; at this density, higher silicon seems to make little difference. In the samples tested the variation of H for one per cent of silicon was as follows:—

Density	2,000	5,000	7,000	8,000
Rockford	5.5	7.3	13.2	19.0
Bessemer	3.7	9.3	14.1	20.8

Also a few tests made on Clinton iron agree in general with the above.

ON THE BRIGHTNESS OF PIGMENTS BY OBLIQUE VISION. By Prof. FRANK P. WHITMAN, Cleveland, Ohio.

THE pigments studied were chosen from the colored papers issued by the Milton Bradley Company of Springfield, Mass. They were compared by the flicker method with a white surface illuminated by a standard light.

Measurements were made at angles of 0, 30, 50 and 70 degrees from the optic axis. The conditions were about as in ordinary photometric work.

The general results found were that the colors of long wave-lengths (reds and yellows) decreased regularly in brightness, while those of short wave-lengths (greens and blues) increased regularly as the angular distance from the optic axis increased. A pigment was found lying between yellow and green in color, which maintained the same brightness at all obliquities.

A METHOD OF MAKING VERY SMALL RODS OF ANTIMONY AND BISMUTH FOR A THERMOPILE. By ROMYN HITCHCOCK, New York, N. Y.

THE metal is pulverized and placed in a glass tube, which is then heated and drawn out. The molten metal fills the fine tubes thus formed. The glass is dissolved away by hydrofluoric acid.

SOME MEASUREMENTS OF DIELECTRIC STRENGTH. By Prof. THOMAS GRAY, Terre Haute, Ind.

THESE measurements were made on a number of solid and liquid substances for the double purpose of determining the strength and whether the ratio of strength to thickness is constant. Some experiments were

also made on the dielectric strength of air for the purpose of finding whether the results of measurements with alternating E. M. F. agree with the older results obtained by electrostatic methods. In these experiments an alternating E. M. F. following approximately the sine law was used. The frequency was about 8000 complete cycles per minute. The E. M. F. was transformed up from one thousand volt mean E. M. F. to the required value by means of a series of four ring transformers each containing two primaries and ten secondaries. The ratio of transformation when all the secondaries were used was 100. The maximum E. M. F. obtainable was thus about 140,000 volts. A series of transformers has some advantages over a single transformer in that there is a smaller difference of potential between the terminals of each one and also that when less than the whole series is applied to the sample those not used act by their inertia to prevent excessive current flowing through.

The results showed that the general rule is a variable ratio between thickness and strength. The strength of air for alternating currents is almost identical with that indicated by the electrostatic experiments of Kelvin, De la Rue, Baille and Freyberg.

The following short table indicates the values obtained for a number of substances. In the case of oils no attempt is made to give the effect of thickness of layer. The strength of the oil varied very much during the test and hence the results are to be taken as only rough approximations:

SUBSTANCE.	THICKNESS CMS.	STRENGTH IN KILO-VOLTS PER CM.	THICKNESS CMS.	STRENGTH IN KILO-VOLTS PER CM.
Mica	0.001	2000	0.10	610
Empire cloth (1)	.033	165	0.23	111
" " (2)	.02	210	0.30	117
" " (3)	.04	145	0.40	97
Empire paper	0.013	450	0.065	256
Paraffined paper	from 400 to 600 kilovolts and nearly constant.			
Hard rubber	.10	540	0.20	430
Soft rubber	.064	492	.25	340
Crystal glass[1]	.100	285	.60	168
Window glass	.200	160	—	—
Fuller board	.025	234	.175	208
Air	.02	56.5	1.00	29.8

[1] This is a trade name for a kind of glass used for lantern slide cover plates. The glass does not contain lead as the name might imply.

A NEW INSTRUMENT FOR THE MEASUREMENT OF THE INTENSITY OF SOUND. By Prof. A. G. WEBSTER and Dr. B. F. SHARPE, Worcester, Mass.

ONE mirror of a Michelson refractometer is fixed to the center of a circular plate provided with a resonator. By a stroboscopic method the displacement of the fringes is measured, when the plate is acted upon by sound. The motion of the fringes is also recorded by photography, and in this manner the constants of the instrument necessary for reduction to absolute measure are obtained.

A NEW CHRONOGRAPH AND A MEANS OF RATING TUNING-FORKS. By Prof. A. G. WEBSTER, Worcester, Mass.

A CHRONOGRAPH driven by a small electric motor of special construction whose speed is governed at each revolution by a current interrupted by a tuning-fork. By using different forks and allowing the clock to register, the number of vibrations can be counted with great accuracy.

A GEOMETRICAL METHOD FOR INVESTIGATING DIFFRACTION BY A CIRCULAR APERTURE. By Prof. A. G. WEBSTER, Worcester, Mass.

BY plotting the definite integrals involved in this problem, a spiral similar to Cornu's, but having cusps, is obtained.

A LECTURE-ROOM EXPERIMENT IN ELECTROSTATICS. By W. S. FRANKLIN, South Bethlehem, Pa.

THIS experiment demonstrates the instability of electrically charged liquid films. Melted rosin is poured from an electrically charged ladle having a sharp lip. The rosin is projected in very fine jets, which cool and form extremely fine fibres which float about in the air.

THE EFFECT OF FIBROUS STRUCTURE IN IRON ON ITS CHANGE IN LENGTH WHEN MAGNETIZED. By E. RHOADS, Baltimore, Md.

THE experiments described in this paper have for their object the determination of the effect which the grain, or fibrous structure of sheet iron, exerts upon its changes in length due to magnetization. Strips 3 x 91.5 cm. were cut from a long sheet of transformer iron 0.035 cm. thick, parallel with, and at right angles to, its length. These were bent into tubes around a bar, and measurements were made of the changes in length of each when subjected to a uniform magnetic field in the direction of its length.

HYSTERESIS LOSS IN IRON FOR VERY SMALL RANGES OF INDUCTION. By HENRY S. WEBB, South Bethlehem, Pa.

THE hysteresis loss in a sample of annealed iron wire was determined for very low values of induction by the method of W. S. Franklin. (Physical Review II, 466).

[To be published in the Physical Review.]

ON A NORMAL CURVE OF MAGNETIZATION OF IRON. By S. S. CLARK, South Bethlehem, Pa.

THE curve of B and H for a soft iron wire was determined under the following conditions, viz.: The iron was put through the process of demagnetization by reversals while various constant values of H were acting and the final value of B determined. Also the values of B were measured after violent vibration. The object was to determine a potential energy function of magnetization.

[This paper will be published in the Physical Review.]

A STUDY OF STANDARD METER SCALES RULED ON NICKEL, SILVER AND GLASS. By Prof. D. C. MILLER, Cleveland, Ohio.

AN HARMONIC PIANO AND ORGAN. By LEVI ORSER, Boston, Mass.

EXHIBIT OF AN AUTOMATIC MERCURIAL AIR PUMP DESIGNED BY Prof. E. W. MORLEY. By Prof. D. C. MILLER, Cleveland, Ohio.

AN IMPROVED METHOD FOR RATING TUNING FORKS. By J. O. REED, Ph.D., Ann Arbor, Mich.

STUDY OF ELASTIC FATIGUE BY THE TIME VARIATION OF THE LOGARITHMIC DECREMENT. By Prof. J. O. THOMPSON, Amherst, Mass.

PHOTOGRAPHIC STUDIES OF THE ELECTRIC ARC. By N. H. BROWN, Ithaca, N. Y.

ON THE MEASUREMENT OF ELECTRICAL OSCILLATIONS OF SHORT PERIOD AND THEIR ABSORPTION BY WATER. By Prof. A. D. COLE, Granville, Ohio.

THE EFFECT OF THE SECONDARY ON THE PERIOD OF OSCILLATION IN A PRIMARY CONDENSER CIRCUIT. By J. H. SMITH, Ithaca, N. Y.

AN ACOUSTICAL MICROMETER. By J. O. REED, Ph.D., Ann Arbor, Mich.

AN INSTRUMENT FOR MEASURING RADIANCE. By Prof. KNÛT ÂNGSTRÖM, Upsala, Sweden. Exhibited by E. S. PERRY, Ph.D.

NOTE ON THE TESTING OF OPTICAL GLASS. By Prof. W. S. FRANKLIN, South Bethlehem, Pa.

POLARIZATION IN THE ZN—H_2SO_4 CELL. By Prof. W. A. ANTHONY, Cooper Union, New York, N. Y.

THE purpose of the experiments described in this paper is to show the real cause of what is called polarization in the ordinary single fluid cells.

The cells experimented with were so constructed that air could be excluded, and provision was made for passing gases through them, the gases bubbling through the liquid and escaping by a tube that ended under water.

A Zn — H_2SO_4 — Cu cell gave when first set up 1.03 volts. The circuit was closed through a resistance of three ohms, which was about the internal resistance of the cell. On opening the circuit the E. M. F. was 0.58 volt, but rapidly increased to 1.02. Repeating the experiment, allowing the circuit to remain closed for some time, recovery was less rapid but was greatly hastened by shaking the cell, or by agitating the liquid by allowing an inert gas, as hydrogen or nitrogen, to bubble through it.

This is a very well known fact, and the explanation usually given is that, when the circuit is closed, hydrogen accumulates on the copper, producing a *counter*-E. M. F., which is so much to be subtracted from the assumed normal E. M. F. of the cell. It is further explained that agitation of the liquid washes off this hydrogen and so removes the cause of the reduction of the E. M. F.

The circuit of the cell referred to above was closed for fourteen hours through an external resistance of three ohms, hydrogen gas slowly bubbling through it during the time. On opening the circuit the E. M. F. was 0.53 volts. There was no recovery of E. M. F. with circuit standing open. Agitation of the liquid produced no change. Hydrogen and nitrogen were passed through the cell slowly and rapidly, but produced no change. The E. M. F. remained fixed at 0.53 volt. Substituting oxygen for the hydrogen, the E. M. F. instantly increased, and in ten seconds had reached 1.01 volts. In ten minutes it was 1.04 volts. The cell then behaved like a freshly-set-up cell, recovering its E. M. F. quickly after its circuit had been closed for a short time, and failing to recover only after being kept in operation several hours, air being excluded.

These experiments demonstrate that the high E. M. F. given by the cell when first set up is due to the oxygen dissolved in the liquid, thereby coming into electrical contact with the copper plate, so that that copper plate is, in its relation to the electrolyte, really an oxygen plate, active in relation to the hydrogen ions just as the other electrode, zinc, is active in relation to the SO_4 ions. The E. M. F. of the cell is the sum of the E. M. F.'s due to these two activities.

The reduced E. M. F., due to the operation of the cell, is not due to the accumulation of hydrogen on the copper, but to the exhaustion of the oxygen from the liquid immediately in contact with the copper. The gradual increase in E. M. F. while standing on open circuit is due to the diffusion of the liquid, and the rapid increase produced by agitation is due to bringing fresh liquid containing oxygen quickly into contact with the copper.

A zinc-carbon cell, from which air had been as thoroughly as possible removed by boiling, showed a drop in E. M. F. to 0.52 volt after the circuit had been closed, and the recovery even at first was very slow. Running oxygen through the cell raised the E. M. F. very quickly to 1.22 volts.

A zinc-platinum cell gave 0.77 volt after having its circuit closed. Even after the circuit had been closed for several hours, the E. M. F., measured a minute or two after opening the circuit, was nearly the same. But on arranging the apparatus to measure the E. M. F. immediately upon opening the circuit, it was found to be only 0.54 volt, practically the same as the E. M. F.'s of the zinc-copper and the zinc-carbon cells.

LeBlanc and others have stated that platinum is an inert metal and that the E. M. F. 0.77 volt, given by a zinc-platinum cell, is the E. M. F. that would be obtained if a pure hydrogen plate could be used as the positive electrode.

The above experiment shows that a platinum plate charged with hydrogen gives only 0.54 volt. The higher E. M. F. of 0.77 volt is due to the fact that platinum is *not* inert, but by its attraction for hydrogen ions withdraws these from the solution, thereby raising its own potential above that of the liquid.

Introducing oxygen into the zinc-platinum cell raises the potential quickly to 1.58 volts. That this is higher than the E. M. F. of the zinc-copper or the zinc-carbon cell, is because of the well-known condensation of oxygen upon the platinum, whereby it is brought into more active relation to the hydrogen.

As a result of all these experiments it is apparent that the E. M. F. of a zinc-hydrogen couple is about 0.52 volt. If oxygen could be completely removed from the liquid and from the surface of the copper or carbon electrode, the cell on being set up would give only that E. M. F. Copper and carbon are, therefore, inert electrodes; while platinum, by its attraction for the hydrogen, furnishes an active force that raises the E. M. F. 0.25 volt higher.

ON CERTAIN FORMULÆ RELATING TO CONTINUOUS CURRENT ELECTRIC ARC LIGHTS. By Prof. WM. HARKNESS, U. S. N., Washington, D. C.

The variable elements in an arc lamp are: 1st, the energy resulting from the drop of potential between the carbons, which is most conveniently measured in watts; 2d, the diameters of the carbons, 3d, the interval between the carbons, or, in other words, the length of the arc; and, 4th, the quality of the carbons. As the length of the arc is automatically adjusted by the lamp, and the quality of the carbons is determined by the processes employed in their manufacture, we may confine our attention to the diameters of the carbons and the energy developed between them.

In any continuous current arc lamp, let M be the maximum intensity of the light emitted when the current consumed is W watts, and the diameter of the carbons is d millimeters. Then, according to the experiments of Max Schreihage, made in 1888, when W is constant M is inversely proportional to d, within rather wide limits. As the most obvious effect of d is upon the current density of the arc, we may assume tentatively that when the said density remains constant M is directly proportional to W. In that case d must vary as the square root of W, and therefore we must have

$$M = \frac{n\, W^{\frac{1}{2}}}{d} \qquad (1)$$

where n is a constant to be determined by experiment.

As Schreihage's result cannot be regarded as absolutely conclusive, it seemed desirable to determine both n and the exponent of W by experiment, and for that purpose the tests of arc lamps made at the Paris Exposition of 1881 have been employed. These tests were made on thirteen lamps, but one of them was incomplete. The remaining twelve show values of W ranging from 421 to 5788 watts, and values of M ranging

from 72 to 1960 carcels, and from a careful least square discussion of them all they were found to be best represented by the formulæ

$$M = 0.0575\ (1 \pm 0.068)\ \frac{W^{1.60}}{d}\ \text{carcels.} \qquad (2)$$

As I am not aware of any other series of tests giving so great a range of W, the exponent of that quantity here found was accepted as final for the remainder of this investigation, and it was sought to improve the value of n by means of the tests of arc lamps made at the Antwerp Exposition of 1885. These tests were twenty-eight in number, but for various reasons I have rejected three of them. The remaining twenty-five show values of W ranging from 154 to 983 watts, and values of M ranging from 52 to 557 carcels, and they give

$$M = 0.0759\ (1 \pm 0.0208)\ \frac{W^{1.60}}{d}\ \text{carcels.} \qquad (3)$$

$$M = 0.6829\ (1 \pm 0.0208)\ \frac{W^{1.60}}{d}\ \text{English candles.} \qquad (4)$$

$$\text{Watts per carcel} = 13.18\ (1 \pm 0.0208)\ \frac{d}{W^{0.60}} \qquad (5)$$

$$\text{Watts per candle} = 1.464\ (1 \pm 0.0208)\ \frac{d}{W^{0.60}} \qquad (6)$$

The probable error of representing any one of the observed values of M by formulæ (3) or (4) is 10.4 per cent.

So far as I know, the only other tests available for determining the value of n are those made by Mr. L. B. Marks, at Cornell University, in 1890. They are seven in number; in them $W = 405$ and 645 watts, M ranges from 70 to 131 carcels, and they give

$$M = 0.0802\ (1 \pm 0.0888)\ \frac{W^{1.60}}{d}\ \text{carcels.} \qquad (7)$$

The numerical values of n given in formulæ (2), (3) and (7) should probably be regarded as an indication of the quality of the carbons employed; and, if so, there was evidently a great improvement between 1881 and 1885, and very little between 1885 and 1890. Until further experiments are made, formulæ (3) to (6) should doubtless be adopted to the exclusion of (2) and (7).

If, in any continuous current arc lamp, we put d for the diameter of the carbons in millimeters, and W for the strength of the current in watts = volts × amperes; then average practice respecting the diameters of the carbons can be well represented by an empirical expression of the form

$$d = m \sqrt[3]{W} \qquad (8)$$

For m, I find the following values from the sources specified:

Tests at the Paris Exposition of 1881,	$m = 1.314 \pm 0.0305$
Tests at the Antwerp Exposition of 1885,	1.515 ± 0.0237
Tested by L. B. Marks in 1890,	1.631 ± 0.0394
Light on Eiffel Tower, Paris, 1889 (7000 watts),	1.516
Hanstholm Light-house, Denmark (9000 watts),	1.538
Washington street lamps, before 1896,	1.472
Washington street lamps, since 1897,	1.682

The whole probable range is covered by the formulæ

$$d = (1.52 \pm 0.16) \sqrt[3]{(\text{Watts})} \tag{9}$$

For the light emitted by any continuous current arc lamp, let H = the horizontal intensity, M = the maximum intensity, S = the mean spherical intensity. Then, in accordance with the empirical formulæ introduced by Gérard in 1881, and now generally accepted by electrical engineers,

$$S = \tfrac{1}{2} H + \tfrac{1}{4} M \tag{10}$$

and if we put

$$\frac{M}{H} = n$$

we shall have

$$S = H \frac{n+2}{4} = M \frac{n+2}{4n} \tag{11}$$

$$M = S \frac{4n}{n+2} = Hn \tag{12}$$

$$H = S \frac{4}{n+2} = M \frac{1}{n} \tag{13}$$

From the fifty-six lamps tested respectively at the Paris Exposition of 1881, the Philadelphia Exposition of 1884, the Antwerp Exposition of 1885, and by L. B. Marks in 1890, I find

$$n = \frac{M}{H} = 2.416$$

and the substitution of that value in the expressions (11) to (13) gives the following results, which may be adopted with considerable confidence:

$$S = 1.104\ H = 0.4570\ M \tag{14}$$

$$M = 2.188\ S = 2.416\ H \tag{15}$$

$$H = 0.9058\ S = 0.4139\ M \tag{16}$$

A STUDY OF GALVANIC POLARIZATION. By BARRY MACNUTT, South Bethlehem, Pa.

THE following electrolytic cells were set up, viz.: $Cu - CuSO_4 - Cu$, $Ag - AgNO_3 - Ag$, and $Zn - ZnSO_4 - Zn$, and their galvanic polarization was determined for a decreasing series of currents, the object being to find if the polarization approaches zero with the current. The results seem to show that the polarization does not approach zero with current and that the process, which takes place in an electrolytic cell when infinitesimal current passes, is not a thermodynamically reversible process.

[This paper will be published in the Physical Review.]

A NEW GAS. By CHARLES F. BRUSH, Cleveland, Ohio.

THE purpose of this purely preliminary paper is to announce the discovery of a new gas; presumably elementary, and possessed of some extraordinary properties. It is a constituent of the atmosphere, and is occluded by many substances. Its chief characteristic thus far ascertained is enormous heat conductivity at low pressures. In order to appreciate this phenomenon, it is necessary to consider the heat conductivity of some of the well-known gases.

A year ago, I had the honor to read before this Section a paper on the transmission of heat by gases, and illustrated by numerous curve sheets showing the heat conductivity of several gases at all pressures, from atmospheric down to the best vacuum obtainable; also an allied paper on the measurement of small gaseous pressures. Abstracts of these papers appear in the current volume of Transactions, and their full text may be found in the Philosophical Magazine for Jan. 1898, and Nov. 1897, respectively.

The apparatus used for the described experiments in heat transmission, consisted in part, of a long-stemmed thermometer hanging in a long-necked glass bulb, the bulb of the thermometer being in the center of the glass bulb. Two bulbs were used for different experiments: the larger one 112 mm. in diameter, the smaller one only 20 mm. A tank of water and crushed ice under the bulb was adapted to be raised when desired, so as to immerse the bulb in the cold mixture. The neck of the bulb was connected with an air pump capable of reducing the internal pressure to a very small fraction of a millionth of atmospheric pressure; also with an elaborate pressure gauge adapted to measure small pressures with very great precision, and a barometric gauge for measuring larger pressures.

In using this apparatus, the gas to be tested was introduced at atmospheric pressure; the ice tank was raised, and the falling temperature of

the thermometer, which could lose heat only by radiation, conduction and convection through the surrounding gas, was observed through a telescope. The time required for the temperature to fall through a given range, usually from 15° to 10° Centigrade, was carefully noted. Then the ice tank was lowered, permitting the thermometer to regain the temperature of the laboratory; some of the gas was pumped out, and the cooling of the thermometer again observed at this reduced pressure. This process was repeated many times, until the pressure was reduced to the lowest point attainable.

The results obtained with each gas were plotted in a curve showing its heat conductivity at all pressures from atmospheric down; the ordinates representing the reciprocals of the time of cooling in seconds, while the abscissas represented the pressure.

The present chart shows curves representing the heat conductivity of several gases, from fifty millionths of atmospheric pressure downward. The data for all of these except the helium curve, are taken from last year's paper; but the scale is different.

I am indebted to Professor Ramsay for the helium used in obtaining the curve here shown.

The ordinates of each curve measured from *A B* as a base line, represent the total rate of heat transmission by the ether and the gas at the pressures indicated by the abscissas; while ordinates measured from the line *C D*, represent the heat transmitted by the gas alone.

It will be observed that the curves of all the gases named vanish together at the point of zero pressure *D*. Repeated experiments have shown this condition to be always strictly true within the narrow limits of errors of observation, *provided* that before the introduction of the gas to be tested, the whole apparatus has been kept highly exhausted for several days; or, better still, has been heated several hours by means of air and water baths, while kept exhausted. Without one of these precautions, I was never able to get any gas curve quite down to the point *D*, for reasons which will appear. The point *D* represents a period of three hundred seconds required for the temperature of the thermometer to fall from 15° to 10°, with the pressure of the surrounding gas reduced to one twenty millionth or less.

A very brief account of the circumstances which led to the discovery of the new gas may not be out of place. I had long been engaged in high vacua experiments, and had observed that glass apparatus when highly exhausted and heated, evolved gas for an indefinite length of time—rapidly at first, then slower, but never stopped until the temperature was reduced. On cooling, rapid reabsorption always took place, but was never complete; indicating that two or more gases had been evolved by heating, one of which was not absorbed on cooling. In other words, the absorption was selective. The truth of this conclusion was abundantly demonstrated subsequently. However, the percentage of reabsorbed gas was so large, that I used a small quantity of pulverized glass in several experiments, to absorb a part of the residual gas after the highest attainable exhaustion

was reached. The pulverized glass was always lead glass like that of the apparatus, and was heated nearly red hot for several hours before and during the final exhaustion.

During these experiments a curious phenomenon was noted; the pulverized glass lost its snowy whiteness. This I thought must be due to reduction of some of its lead; probably by hydrogen evolved on heating. If so, I argued that the hydrogen must have been absorbed from the atmosphere since the manufacture of the glass; and its presence in the glass if proven, would be a long step toward proving the normal presence of hydrogen in the atmosphere.

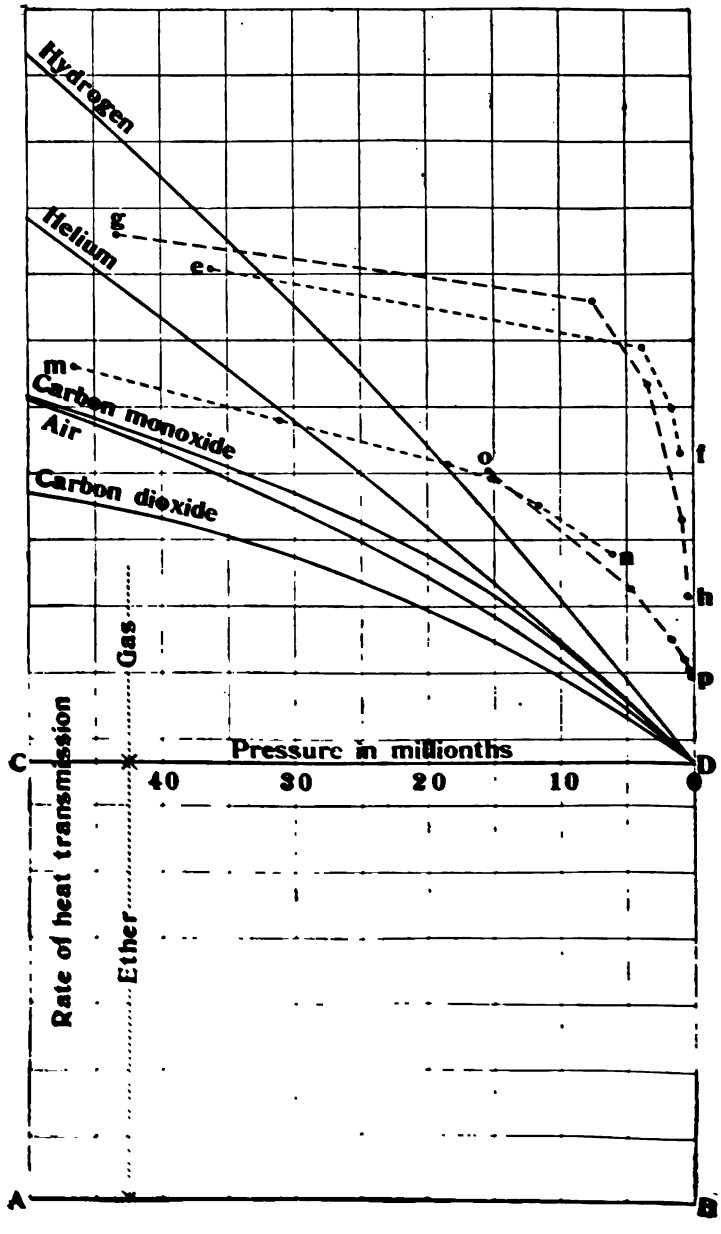
Following the line of experiment thus suggested, I prepared a quantity of pulverized soda glass free from lead, and placed about 120 grammes of it in a large combustion tube connected with the apparatus already described. The glass was made from an old stock of tubing, and was of such a degree of fineness that it all passed through a sieve of 90 meshes to the linear inch, but would not pass through a 140-mesh sieve. The combustion tube was adapted to be heated by a gas furnace with automatically regulated gas supply, so as to be maintained at any desired temperature.

While prepared to make analysis in the usual way, of the gas evolved by the pulverized glass in case it appeared in sufficient quantity, I relied chiefly on its heat conductivity for the detection of any large proportion of hydrogen in the last few millionths, not being prepared at that time for spectroscopic examination; and thinking that perhaps some hydrogen might be evolved in the last stages of exhaustion before heating, I tested the conductivity of the residual air from time to time as the preliminary exhaustion progressed.

When the exhaustion approached a good vacuum, an astounding phenomenon developed. At 36 millionths pressure the residual gas conducted heat twice as well as air, and nearly as well as hydrogen; at 3.8 millionths it conducted seven times as rapidly as hydrogen; at 1.6 millionths, fourteen times, and at .96 of one-millionth, twenty times as rapidly. These results are plotted in the curve *e f*. I did not carry the exhaustion lower than .96 M. At this pressure the time of cooling of the thermometer from 15° to 10° was only 177 seconds, while pure hydrogen would have required 288 seconds.

Evidently a new gas of enormous heat-conducting capacity was present, mixed with the last small portion of air. It must have come from the pulverized glass, and probably formed only a small fraction of the mixture. The last cc. of gas pumped out was collected, and upon subsequent analysis of it, nothing but air was found.

The discovery of the new gas as above outlined, was made nearly a year and a half ago, on March 10, 1897. On the following day, the pressure had increased to nearly five millionths; while the time of cooling of the thermometer, instead of diminishing as usual with increase of pressure, had raised from 177 seconds to 245 seconds, showing that the new gas had largely disappeared. This was subsequently found to be due to absorption by the phosphorous pentoxide used in desiccation.


Hydrogen
Helium
g
e
m
Carbon monoxide
Air
Carbon dioxide
o
d
a
f
h
p
Gas
Rate of heat transmission
Pressure in millionths
C
D
40
30
20
10
Ether
A
B

Long-continued moderate heating of the pulverized glass caused the evolution of some air, much carbon dioxide and hydrogen, some carbon monoxide, and more of the new gas. The selective absorption which occurred on cooling, was confined almost wholly to the hydrogen and new gas. After further moderate heating and thorough exhaustion, I raised the temperature, continued the exhaustion, and got the results embodied in the curve *g h*, showing that much of the new gas was present; the conductivity at .38 millionth, the last station in the curve, being *twenty-seven times* that of hydrogen. Several days of high heating, with frequent exhaustion, failed to deprive wholly the pulverized glass of its new gas, though the output was greatly diminished. For this and other reasons, I believe that the new gas resides *in* and not simply on the surface of glass.

To make sure that the apparatus was not deceiving me, I sealed off the combustion tube, heated the vacuous parts several hours by means of air and water baths, as before indicated, admitted air dried over phosphorous pentoxide, exhausted step by step, and got the data for the "small bulb" air curve shown here and in last year's paper. Evidently I had not been deceived about the presence of a new gas in the pulverized glass. I subsequently exposed this lot of glass several days to the atmosphere, spread out in a thin layer. When tested again after this exposure, it yielded the new gas both before and after heating, as freely as at first. This rejuvenation of pulverized glass by exposure to the air, was fully confirmed with another lot made from common window glass; and in connection with the results obtained by the diffusion of air, hereafter described, leaves no room for doubt that the new gas is a constituent of the atmosphere.

Pulverized glass appears to begin evolution of the new gas as soon as the atmospheric pressure is reduced. In one case, while making a slow preliminary exhaustion, I tested the air which remained when the pressure was still 132 mm.; and to my great surprise, found it contained not only a trace, but a considerable quantity of new gas. The amount increased rapidly as the pressure was further reduced. This leads to the belief that finely pulverized glass gives up the greater part of its new gas on simple reduction of pressure to a low point, and without heating; somewhat as palladium gives up occluded hydrogen.

Several other substances than glass were examined, and nearly all found to contain the new gas. A specimen of old charcoal made from pine-wood sawdust by long exposure to a bright red heat, yielded as was expected, comparatively large quantities of several gases when heated in vacuum. At some stages of the evolution these were rich in the new gas.

It would seem easy in such cases to remove the diluent gases by oxidation and absorption; but it is not. After making the necessary additions to my apparatus, I made many attempts of this kind. The reagents used were in large excess on account of the very small quantities of gas treated, so that some of the observed results may have been due to impurities. Cupric oxide and lead chromate evolved gases of their own

almost indefinitely on high heating in vacuum, and then freely absorbed carbon dioxide and moisture at a lower temperature. Soda-lime dried in vacuum in presence of phosphorous pentoxide, was almost indifferent to carbon dioxide. All the reagents named, especially phosphorous pentoxide, absorbed the new gas; and all but the latter gave it up again on heating.

A specimen of very fine white silicious sand when heated in vacuum, gave a large quantity of gas consisting principally of hydrogen and hydrocarbons, with a considerable amount of the new gas. Some of this mixture was exposed successively to the action of red hot cupric oxide, soda-lime, and phosphorous pentoxide. By this treatment the gases were reduced to less than three per cent of their former volume. The residue was not very rich in the new gas, because of the absorption of the latter by the soda-lime and phosphorous pentoxide, as I afterwards learned; but I have shown the curve *o p* of its heat conductivity, because the pressure was carried to a lower point than in any other case. At the last station in the curve, representing a pressure of .12 of a millionth, the conductivity was equal to that of 5.1 millionths, or forty-two times as much hydrogen. From this it seems reasonably certain that the curve *e f*, if carried to as low a pressure, would have shown a conductivity at least a hundred times greater than hydrogen. And yet the new gas in that experiment must have been very far from pure on account of the continuous evolution of ordinary gases, as shown by the rise of pressure and loss of conductivity during the next few hours.

It will be observed that when the new gas was present, the form of the conductivity curves was very different from those of the known gases; the effect of the new gas becoming more prominent as the pressure was reduced. I do not doubt that this was due to the interference of the heavier and slower-moving molecules of the ordinary gases always mixed with the new gas. In last year's paper I described the same effect found in a mixture of carbon dioxide and hydrogen. When the new gas is obtained in a state of purity, I expect to find its curve of heat conductivity similar to those of hydrogen and helium, but with immensely increased ordinates.

Believing that the new gas is very much lighter than air or hydrogen, and may therefore be separated from the atmosphere by successive diffusions, I have spent several months in experiments with this end in view. Quite recently my efforts have been crowned with most promising success. The difficulty has been to find a suitable porous septum; one free from holes, and sufficiently fine-grained to prevent more than a very slow passage of air, while a considerable surface is exposed to the atmosphere on one side, and to a fairly good vacuum on the other. Many substances were tried. India-rubber gave encouraging results, but was capricious, and very slow. The best results have been obtained with porous porcelain having its superficial pores nearly closed by suitable treatment. A tube of this kind, closed at one end, and exposing rather more than five square inches of surface to the air, was connected with the apparatus, and the whole kept exhausted to a pressure of 1.3 mm. About 19 cc of gas was diffused per hour. After thirty-six hours the diffusion tube was shut off,

the exhaustion continued, and data for the curve *m n* obtained. It was found impracticable to carry the exhaustion below six millionths because of the presence of moisture, which interfered with the action of the pump, no desiccating agent being used. As both phosphorous pentoxide and soda-lime absorb the new gas, they cannot be used for desiccation. If the exhaustion had been carried as far as in the curve *o p*, it is probable that a higher conductivity than in that curve would have been shown; *i. e.*, a conductivity more than forty-two times that of hydrogen, or something like a hundred times that of air. Here we have the heat conductivity of air at very low pressure, increased something like a hundredfold by one diffusion. I have not yet tried a secondary diffusion, but feel confident that successive diffusions of air will afford a practicable means of obtaining the new gas in a state of approximate purity.

Now let us discover, if possible, the meaning of the extraordinary heat conductivity of the new gas. I have tabulated the molecular weight, density, specific heat, mean molecular velocity, and heat conductivity of hydrogen, helium, carbon monoxide, air, and carbon dioxide, which are the known gases represented in the curve chart. For easy comparison, I have taken not only the density, but the specific heat, mean molecular velocity, and heat conductivity of hydrogen as unity. The similarity of values in the fifth and sixth columns, for molecular velocity and heat conductivity, is striking. On the chart, the curves for carbon monoxide, air, and carbon dioxide, are evidently too high to correspond well with the relative values of molecular velocity in the fifth column; but these curves represent only the last 50 millionths of the complete curves. By following these back to 500 millionths, which is still a rather small pressure, and taking their values at intervals of 10 millionths from no pressure upward, we obtain as a mean of all these values for each gas, the quantities shown in the sixth column. These agree fairly well with the molecular velocities.

COMPARISON OF GASEOUS PROPERTIES.

1	2	3	4	5	6
Gas	Molecular weight	Density D	Relative specific heat	Relative molecular velocity $\frac{1}{\sqrt{D}}$	Relative heat conductivity
Etherion	? 0.0002	? 0.0001	6000.?	100.?	100.
Hydrogen	2.	1.	1.	1.	1.
Helium	4.	2.	?.300	0.71	0.73
Carbon monoxide	27.8	13.9	0.072	0.27	0.33
Air	28.8	14.4	0.069	0.26	0.32
Carbon dioxide	43.8	21.9	0.064	0.21	0.21

I offer the suggestion that the relatively high conductivity of the last three gases at the low pressure shown in the chart, is due to the dissociation of some of their molecules by unobstructed impact on the walls of the containing vessel; recombination being more and more retarded as the pressure is lessened, because the greater separation of the molecules reduces the frequency of collisions. Helium, perhaps because it is monatomic and therefore incapable of dissociation, has a nearly straight curve like hydrogen; and its ratio, given in the sixth column, varies but little throughout the whole range observed, which was more than 1,000 millionths. I have taken the density of helium as 2; but Professor Ramsay finds it a little less than this when purified as far as possible by repeated diffusions. This makes its relative molecular velocity a little more than .71, and brings it still closer to the observed value of its heat conductivity.

From the foregoing, we may reasonably conclude that the heat conductivity of gases at low pressures, and their mean molecular velocities, are closely related. Hence, if we can learn the heat conductivity of an otherwise unknown gas, we can form some idea of its mean molecular velocity; and from this, of its specific heat, density, and molecular weight.

As before indicated, the heat conductivity of the new gas at very low pressure, even when mixed with a large proportion of other gases, is something like a hundred times that of hydrogen. I shall not be surprised to find the conductivity of the pure gas a thousand or more times greater than that of hydrogen; but let us be conservative, and for the present purpose call it a hundred times, and see what follows. I have given it this value at the head of the sixth column.

If my inference that the heat conductivity and molecular velocity of gases are directly related is correct, then the molecular velocity of the new gas will be one hundred times that of hydrogen, as shown at the head of the fifth column. As is well known, the mean molecular velocity of a gas varies directly with the square root of its absolute temperature, and is independent of pressure. The mean molecular velocity of hydrogen at the temperature of melting ice, has been found by calculation to be 5,571 feet per second. Hence the mean molecular velocity of the new gas at the same temperature will be 557,100 feet, or more than 105 miles per second. At anything like this molecular velocity, it would be quite impossible for a gas to remain in the atmosphere, *unless the space above also contained it.* A velocity of only about seven miles per second, if unchecked except by gravitation, would be sufficient to project a body from the earth permanently into space. Even at a temperature very close to absolute zero, the new gas would have sufficient molecular velocity to escape from the atmosphere.

Again, inasmuch as the molecular velocities of gases vary inversely as the square roots of their densities, it follows from our assumption of its molecular velocity, that the density of the new gas is only the ten thousandth part that of hydrogen, as shown at the head of column 3. This is the hundred and forty four thousandth part the density of air. It is

generally believed that the gases of the atmosphere distribute themselves in the long run, each as though the others were absent. Hence the new gas must extend a hundred and forty four thousand times as high as the heavy constituents of the air, to bring about the same proportionate reduction of pressure, even if gravitation remained constant at all distances from the earth; but the restraining influence of gravitation on the expansion of the atmosphere diminishes as the square of the distance from the earth's center increases. It is evident therefore, without a mathematical demonstration, that the new gas being present in the atmosphere, must extend indefinitely into space without great loss of pressure. This is only another way of stating the result of its assumed molecular velocity. Now there is no doubt that the new gas exists in the atmosphere, though probably in very small proportion; perhaps much less than a millionth. Hence it seems really probable that it not only extends far beyond the atmosphere, but fills all celestial space at a very small pressure. In recognition of this probability, I have provisionally named it ætherion or etherion; meaning "high in the heavens." Its symbol will naturally be Et.

I am aware that strong objections may well be raised to the hypothesis of an interplanetary and interstellar atmosphere; but I can see no escape from the conclusion I have drawn, if I am not mistaken in my premises.

The estimated relative specific heat of etherion appears at the head of the fourth column, based again on the assumed relative molecular velocity. In estimating the specific heat, I have not made it inversely proportional to the density, as would be required by Dulong and Petit's law, giving a value ten thousand times that of hydrogen; but have used the formula suggested by Professor Risteen in his work on "Molecules and the Molecular Theory," which requires that the product of the specific heat and molecular weight of gases shall vary with the number of degrees of freedom of their molecules. I have assumed as probably true, that etherion is monatomic, with atoms possessing only three degrees of freedom.

Of course the values I have estimated for the molecular weight, density, specific heat, and molecular velocity of etherion, are intended only to indicate the *order* of magnitudes we may expect to find on further investigation; and it must not be forgotten that they are based on two assumptions: first, that the heat conductivity of etherion is one hundred times that of hydrogen, and second, that the ratio of heat conductivity and mean molecular velocity is the same for all gases. As before indicated, I expect to find the heat conductivity of etherion much higher than the value here assigned to it. If so, the real values of its other attributes will be still more startling than those here given. The second assumption, while by no means proven, seems at least a good "first approximation" to the relation between heat conductivity and molecular velocity in gases.

There is some evidence that etherion is a mixture of at least two different gases. In the course of my experiments I have met with a great many specimens, obtained in various ways from various sources; but always mixed with a very large excess of other gases. Some specimens were almost wholly absorbed by the phosphorous pentoxide at first used

for desiccation. Others were but partially absorbed; the absorption being very rapid at first, but in an hour or two dwindling to nothing, and leaving a residue of gases permanently showing by their heat conductivity, the presence of a very considerable amount of etherion. Soda-lime absorbed etherion, but much less freely than phosphorous pentoxide, and gave it up again on heating. The gas thus recovered was but little, if at all, affected by phosphorous pentoxide.

In one experiment, the gases evolved from ten ounces of pulverized window glass, both before and after heating, were passed through coarsely pulverized soda-lime and then over fresh phosphorous pentoxide. Not a trace of etherion remained. The same result was obtained when another lot of the silicious sand already referred to, was used as the source of etherion.

I will venture the conjecture that etherion will be found to consist of a mixture of three or more gases, forming one or more periodic groups of new elements, all very much lighter than hydrogen. If this prove true, I propose to retain the present name for the lightest one.

The transmission of radiant energy through space has always been to me a fascinating phenomenon, and I have indulged in much speculation concerning the ether—that mysterious something, by means of which it is effected. The remarkable properties assigned to the ether from time to time, in order to account for observed phenomena, have excited my keen interest; but I have long entertained the hope that some simpler explanation of the mechanism involved will be found. To me, a less strain of the imagination is required in the assumption that instead of a continuous medium, gaseous molecules of some kind, endowed perhaps with a mode or modes of motion at present unknown to us, are the agent of transmission; a gas so subtle, and existing everywhere in such small quantity, that it has escaped detection.

Perhaps the molecular hypothesis of the ether has proven so attractive to me, because it supports the hope that we may sometime compass the perfect vacuum—a portion of space devoid of *everything*. Such a vacuum would be opaque to light, and gravitative attraction could not, I believe, act through it. It might afford a new point of view from which to study the profound mystery of gravitation — an *outside* point.

The late De Volson Wood (Phil. Mag. Nov. 1885) considered the question of a gaseous ether mathematically, and deduced certain necessary properties of the hypothetical gas, chief among which were exceedingly small density, and exceedingly high specific heat. Possibly we are about to find a gas which will fulfil the required conditions. It may be etherion, or its lightest constituent, if it turn out to be a mixture. I venture to express the hope that etherion will at least account for some phenomena at present attributed to the ether.

On account of the presumably extreme smallness of its molecules as compared with those of glass, etherion probably passes through the latter when any considerable difference of pressure exists on opposite sides though the passage may be very slow. It seems to be condensed or com-

pressed in glass as before indicated, and may evaporate on the side of lower pressure, and be absorbed on the side of higher pressure, after the manner of hydrogen in passing through palladium. In my own experiments, the heat transmission ascribed to the ether may be due to the presence of the new gas inside the bulb. A small fraction of a millionth would be sufficient, and this might escape detection by the pressure gauge on account of the necessary compression in the gauge head causing absorption by the glass. Again, etherion must always be present to some extent in all "vacuum tubes" (as well as in my own conduction bulb), on account of its long-continued evolution from glass, and may be the medium of propagation of the Roentgen rays in the vacuum glass and air.

POLARIZATION AND INTERNAL RESISTANCE OF ELECTROLYTIC CELLS. By Dr. K. E GUTHE, University of Michigan, Ann Arbor.

A NUMBER of careful experiments with constant and alternating currents have shown that the apparent resistance of an electrolytic cell or a battery decreases with increasing current, and this has led in some cases to the assumption of a resistance of transition (Uebergangswiderstand). The author showed that for electrolytic cells, Cu in $CuSO_4$ solution, this change of the apparent resistance can easily be explained by polarization. Wiedeburg's theory was adopted, which derives for the polarization of a simple electrolytic cell with electrodes of the same size

$$p = P\,(1 - e^{-\frac{\beta}{s}q})$$

The application of this formula to the case in question gives for the real resistance of the cell, if the time during which the current flows is kept constant,

$$r = \left[E' - P\,(1 - e^{-kI})\right]\frac{1}{I}$$

In the experiments the disturbing influences of absorption and convection were practically eliminated.

The results show that r is a constant and that the electric action in such a cell can be expressed very well by Ohm's law, taking into account the polarization.

It is apparent that the method can be applied to the more complicated case of a voltaic battery.

[To be published in the Physical Review, Sept.-Oct. 1898.]

A NEW DETERMINATION OF THE ELECTROCHEMICAL EQUIVALENT OF SILVER.

By GEO. W. PATTERSON, JR., M.A., B.S. and KARL E. GUTHE, Ph.D., University of Michigan, Ann Arbor, Mich.

THE discrepancy of the values for the mechanical equivalent of heat obtained by the electrical method and those found by the direct method[1] has suggested the possibility of the difference being due to an error in the standards used. The value of the ampere was suspected to be wrong and this led us to attempt a redetermination of its value by a new method. The electrochemical equivalent of silver has been repeatedly determined and was found to be .0011156 grm by Mascart;[2] .00111794 grm by Lord Rayleigh and Mrs. Sidgwick[3] and 0011183 grm by Fr. and W. Kohlrausch.[4] In the first two cases an electrodynamic balance was used; in the last an accurate tangent galvanometer. Heydweiller[5] compared Lord Rayleigh's and Kohlrausch's methods directly and came to the conclusion, that the error in the two methods could not be larger than .1%. Köpsel[6] found .0011174 grm with a balance built according to von Helmholtz's directions. The experiments by Pellat and Potier,[7] using Pellat's absolute electrodynamometer, gave .0011192 grm. For a number of years Kahle has been at work at the Imperial Institute in Berlin using Helmholtz's current-balance[8] but the definite report of this valuable investigation has not yet been published. In a preliminary report he gives .0011182 for the equivalent, when the silver was deposited from a freshly prepared solution, but .0011193, when he used an old solution.[9]

In all the experiments mentioned the value of the electro-chemical equivalent of silver depends upon the accurate knowledge either of the acceleration due to gravity or of the horizontal intensity of the earth's magnetic field. We proposed to measure its value entirely independent of g or H by balancing the moment due to the electrodynamic action of the two coils of an absolute electrodynamometer, designed by us and made under our direction, against the torsional moment of a wire with very small elastic fatigue. The measurements are finally reduced to measurements of mass and length only and we believe our result to be accurate to at least one in five thousand.

[1] A table with the results obtained by various methods since 1867 is given in Phil. Mag. 33, 160, 1895; see also Day, Phys. Rev. VI, 193, 1898 and Johns Hopkins Univ. Circ. XVII, 54, 1898.

[2] Journ. de Phys. (2) 1, 109, 1882-3, 1883, 1884.

[3] Proc. Roy. Soc. 37, 144, 1884, or Phil. Trans. 2, 411, 1884.

[4] Sitzungsber. d. phys.-med. Ges. Würzburg 1884, Wied. Ann. 27, 1, 1886.

[5] Inaug.-diss., Würzb. 1886.

[6] Wied. Ann. 31, 208, 1887.

[7] Journ. de Phys. (2) 9, 381, 1890.

[8] Wied. Ann. 59, 532, 1896.

[9] Zeitschr. f. Instrum. Kunde 17, 144, 1897, and 18, 141, 1898.

DESCRIPTION OF THE METHOD.

In our determination we made use of Poggendorff's potentiometer method for comparing electromotive forces. First we found the ratio between the E. M. F. of two Carhart-Clark cells immersed in a large water-bath and the potential difference at the terminals of a coil of manganin wire, when a current flowed through it, which just balanced the torsional moment of the suspending wire of the electrodynamometer due to one complete twist of the torsion head. The absolute value of this current was calculated from the instrument.

Let I be the calculated current,
E the E. M. E. of the cells,
R_1 the resistance necessary to balance E,
R_2 the resistance necessary to balance the potential difference due to I,
R the resistance of the manganin coil,

then

$$I = \frac{R_2 E}{R_1 R} \tag{1}$$

Secondly, we balanced the E. M. F. of the cells against the potential difference due to the silver depositing current.

Let I_1 be this current,
R'_1 and R'_2 the resistances necessary for the balances,
E and R the same as before,

then

$$I_1 = \frac{R'_2}{R'_1} \frac{E}{R}. \tag{2}$$

The silver deposited by the current I is

$$q = zI_1 t \text{ grms.}, \tag{3}$$

where z is the electrochemical equivalent of silver

but

$$I_1 = I \frac{R_1}{R_2} \frac{R'_2}{R'_1} \tag{4}$$

therefore

$$z = \frac{q}{It} \frac{R'_1}{R_1} \frac{R_2}{R'_2} \tag{5}$$

We are thus independent of the absolute value of the E. M. F. of the standard cells and the manganin resistance coil.

We chose Carhart-Clark cells, in which the zinc sulphate solution is free from crystals at ordinary temperatures, rather than normal Clark cells, in which the zinc sulphate solution is always in presence of crystals, to avoid all time lag of electromotive force due to changing density after change of temperature. The Carhart-Clark cells have the further advantage of having only about one-half as great a temperature coëfficient as the normal Clark cells.

The manganin coil had a negligible change in resistance within the limits of the temperatures at which we used it. The current through the silver voltameters was not measured directly by the electrodynamometer. The reason for not doing so was that the electrodynamometer was extremely sensitive, and it would have been almost, if not quite, impossible to control the current through the circuit from the beginning to the end of a set of observations, at such a value as to balance the turning moment of the suspension due to exactly one turn of the torsion head of the instrument. This difficulty was due to several causes: first, the current diminished gradually, due to the polarization of the battery and heating of the coils; second, slight changes of temperature produced changes in the wire suspending the movable coil, from which resulted changes in the torsional constant and in the zero point; third, there were gradual though very small changes in the zero point due to elastic fatigue of the suspension. Besides these reasons, it should be mentioned that an absolute electrodynamometer should be used with all four possible permutations of direct and reversed current through both coils.

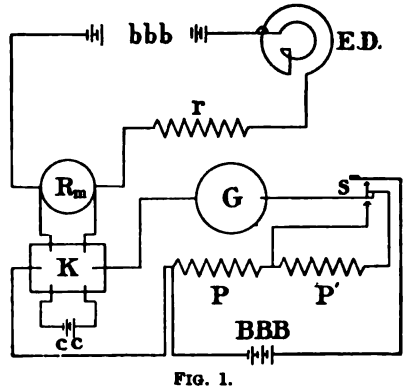


FIG. 1.

Poggendorff's method gave an almost ideal means of holding all these diverse disturbing factors in control. Fig. 1 shows the arrangement of our apparatus.

In our experiments we used either two or three storage cells *BBB* to produce the current through two carefully adjusted resistance boxes P, P' by Nalder Brothers. The total resistance was kept at 11110 ohms, and the fall of potential through a portion P of this resistance, was balanced against the electromotive force of the standard cells *CC* or against the fall of potential through the manganin coil R_m. *K* is a Pohl's commutator, *G* a sensitive Kelvin galvanometer and *S* a successive contact key. In one arrangement the manganin coil was in circuit with the storage battery *bbb* and the electrodynamometer *E. D.* in the other two silver voltameters was included in the circuit also.

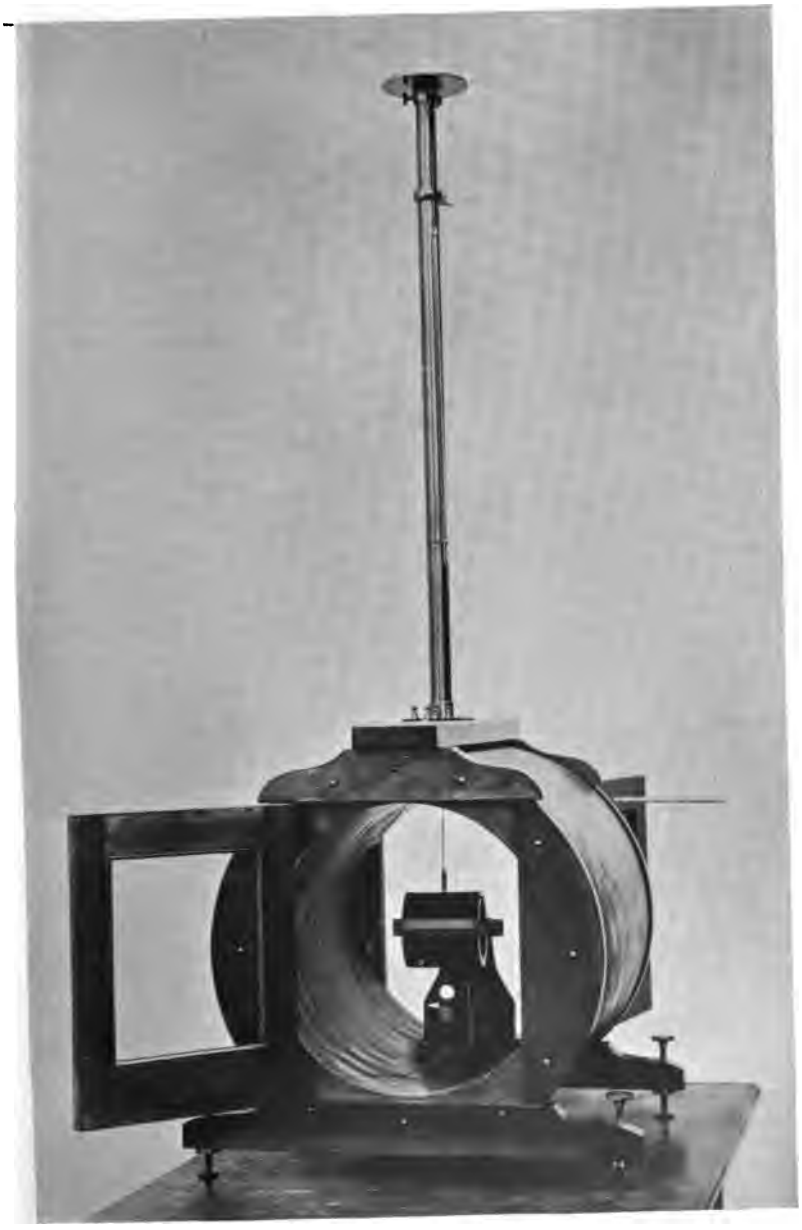


FIG. 2.

The silver nitrate solution was made by weight 15 per cent c. p. silver nitrate and 85 per cent distilled water with a trace of silver oxide added to insure a neutral solution. The same solution was used on successive days. Kahle in a recent paper[1] has called attention to a difference he finds in the electrochemical equivalent of silver when determined from "fresh" and "used" solutions. We did not prepare a solution afresh for each determination. The close agreement between our different results would indicate that our solution remained constant for at least the last four days, on which the experiments were taken that were used for the final calculation. We used for silver voltameters two platinum bowls of 7.5 cms. diameter and 2.5 cms. height as kathodes and pure silver plates about 2.5 cms. diameter wrapped in filter paper and suspended by platinum wires near the centres of the bowls as anodes. We used currents slightly less than one ampere and kept the circuit closed for about an hour. Before using the bowls they were carefully cleaned, dried, and weighed with accurate weights to the nearest tenth of a milligram. After opening the circuit the anode was removed, the solution poured back into its bottle and the bowls carefully washed twice with distilled water and then filled with distilled water and left to soak for a day, during which time the water was renewed once. The bowls were emptied, dried in an air bath at about 130° and left to cool in desiccators. They were then weighed as before.

THE ELECTRODYNAMOMETER. (FIG. 2.)

Our electrodynamometer consisted of a fixed coil of 576 turns in a single layer of 41.633 cms. mean length and 48.118 cms. mean diameter and a movable coil of 45 turns of 8.805 cms. mean length and 10.0187 cms. mean diameter suspended at the centre of the fixed coil by a single phosphor-bronze wire .0355 cm. in diameter and 100.5 cms. long. The current was led into the movable coil through two mercury cups below and in line with the suspension. The torque, due to the current in the fixed coil and the conductors between the movable coil and the mercury cups, was avoided by using a symmetrically divided circuit leading around the upper mercury cup, to the mercury in the lower cup. There was, however, a slight torque due to the action of the current in the conductor leading from the upper cup to the point beyond which the lead wires were twisted together. The lead wires from the terminals of the fixed coil were carried along the surface, parallel to the axis, to binding posts at diagonally opposite corners of a square, and the lead wires from the suspended coil were brought out to the other corners of the square. The action of these lead wires is shown in the difference between the values of the current needed to balance a right-handed or left-handed twist of the suspension. The error due to this asymmetry was eliminated by reversing the current in the fixed coil.

[1]Zeitschr. f. Instr., 17, 144, 1897.

In most cases we used one complete turn of the torsion head, which was determined by means of a mirror on the torsion head and a telescope and scale at a distance of about 150 cms. The torsion head could be read by means of a vernier to one ten-thousandth of a circumference, but we did not in general make readings on it directly, as the telescope, mirror and scale made a much more accurate determination of a complete turn possible.

The cylinder holding the fixed coil was built up of dry, clear pine wood about 2.5 cms. thick. Each layer consisted of five pieces, and successive layers broke joints. The grain of the wood ran around the cylinder. By this arrangement we succeeded in keeping the cylinder of uniform diameter, although the wood shrank a little (one in five thousand) after it was put together. The layers were fastened together by both glue and brass screws. After the cylinder was built up, it was turned in a large lathe to the required diameter. It was then given many coats of shellac, outside and in, to keep it as nearly as possible moisture-proof. It was then wound with 576 turns of number 23 (B & S gauge) single cotton-covered wire which afterwards was given eight coats of shellac to ensure good insulation. The insulation was tested and found perfect. The winding was helical except near the small opening through which the suspension passed, where short portions of six turns were displaced. To make the effect of this displacement as small as possible, three wires on each side of the hole were half sunk into the wood of the cylinder and three others were wound on top of them. By this means the mean radius of the coil was unchanged and the effect on the mean field within the suspended coil was not changed materially. The suspended coil was wound on a hollow vulcanite cylinder. The conductor was a copper ribbon made by rolling a number 25 (B & S gauge) wire down to a thickness of .024 cm. The cylinder was connected to the suspension by means of a small brass rod about 0.2 cm. diameter which was soldered above into a hollow brass cylinder and below into a brass plate which extended the length of the cylinder and was fastened by screws to its ends. Where the brass rod passed from above into the vulcanite it was cut away on two sides so as to allow the coil to keep in a nearly helical form. The suspending wire was soldered into a brass cylinder which was fitted and pinned into the hollow brass cylinder mentioned above. The terminals of the suspended coil were at points along the bottom element of the vulcanite cylinder and were connected along the inner surface: one to the upper mercury cup by a straight wire soldered to a small brass pin, with a platinum wire tip in line with the suspension; the other by a Y-shaped connection to two symmetrical rods which passed out on opposite sides of the upper mercury cup and were soldered into a cross bar which held a brass rod in line with the suspension. This rod supported a mirror and an adjusting weight, and held at its end a platinum wire which projected into the lower mercury cup. The adjusting weight was designed to make the mass of the system exactly equal to the mass whose moment of inertia and period of torsional vibra-

tion determined the torsional constant of the suspending wire. (See Fig. 3.)

The dimensions of both the suspended and fixed coils were intended to fulfil the requirement laid down by Gray,[1] that the length and radius should be in the proportion of $\sqrt{3}:1$. When both coils fulfil this requirement and when their centres coincide, all the terms of the series expressing the torque between the coils disappear except the first, seventh and following odd terms. If the dimensions of the fixed coil are large in comparison with those of the movable coil, all terms after the first may be neglected. Owing to shrinkage of the wood of the fixed cylinder, the fixed coil became somewhat too short to fulfil Gray's requirements; the suspended coil was much closer; nevertheless, the correction necessary does not amount to even one in one hundred thousand.

We carefully levelled the fixed coil by means of a long level. Then we adjusted the zero position of the movable coil by means of a set of concentric squares and circles on a circular board which was fitted into one end of the cylinder of the fixed coil. The suspended coil was turned parallel to the fixed coil and the height of the torsion head was adjusted until the centre of the movable coil was opposite the centre of the squares and circles. The torsion head was then given a half turn to test the level of the movable coil. This was found perfect. Next a scale was placed across the other end of the movable coil and the levelling screws under one side of the instrument were adjusted to bring the axis of the fixed coil and the suspension into the same plane. The adjusting screws at the torsion head were then used to bring the line of the suspension into the middle circle of the large cylinder which corresponded very closely with the middle of the fixed coil. The positions of the mercury cups were then adjusted to bring the platinum tips to their centres. The tests were then all repeated and the position of the movable coil appeared exactly central and horizontal. We then turned the torsion head one-quarter revolution and verified the position by means of the concentric squares and the flat ends of the movable cylinder. Any error in the level of either coil would introduce an error proportional to the difference between unity and the square root of the cosine of the angle. No error of the magnitude of one part in ten thousand could have been present.

The electrodynamometer was set up with the axis of the fixed coil roughly in the magnetic meridian and the axis of the movable coil exactly at right angles to that of the fixed coil. We chose this position to avoid having the movable coil act like a dip needle when the current was on. The effect of the earth's field was considerable, but was eliminated by reversing the current in both coils and adjusting its value until the same torque was produced. Let the current through the coils be represented by i_1 and i_2 in the two cases and let a and b be coëfficients to take account of the dimensions of the instrument and the value of the earth's

[1] Absolute Measurements in Electricity and Magnetism, Vol. II, p. 277, or Phil. Mag. 33, p. 62.

FIG. 3.

FIG. 3.

was reached after ten minutes' twisting. On untwisting, it took a much longer time for the residual twist to disappear. In the use of the electrodynamometer we eliminated the effect of the elastic fatigue by twisting the wire once round and leaving it twisted for several minutes before beginning our observations. The current was adjusted until the movable coil hung in its zero position at right angles to the fixed coil. The circuit was then opened and the torsion head turned back to its zero position. The position of equilibrium of the movable coil was then quickly determined by using an auxiliary battery, and the torsion head was again given a full turn and enough more to allow for the deviation of the position of equilibrium from the zero position. The circuit was then closed and the current adjusted until the movable coil was in its zero position again. The circuit was then broken and the torsion head turned back one complete revolution and the position of equilibrium redetermined. This was repeated until the position of equilibrium was the zero position, or until successive positions lay on both sides of zero. The strength of current was indicated by the number of ohms in the box P of Fig. 1.

The question naturally arises: Did our method of allowing for the slight elastic fatigue introduce any error? We think not. We made a number of observations to test this. We allowed the suspension of the electrodynamometer to rest over night. We then completed the circuit but did not twist the suspension. We adjusted the current to the right value to correspond to a complete turn and allowed the current to become quite steady without twisting the wire. After, say, twenty minutes we opened the circuit, noted the position of equilibrium which in general changed slightly owing to a change in temperature, and closed the circuit again. We then turned the torsion head a complete revolution with the proper correction for the change in the position of equilibrium and determined the number of ohms in P (Fig. 1) required to balance the current and the temperature noted. We kept adjusting the current so as to keep the movable coil in its zero position and noted the corresponding number of ohms in P. We continued this over a period of half an hour or until conditions had become constant. From these observations we estimated by extrapolation what number of ohms in P would have corresponded to a balance at the instant of twisting the suspension. We then opened the circuit, adjusted the zero and determined the ohms corresponding to one complete turn of the suspension from this last zero. This value remained constant except for change of temperature, showing that the change due to elastic fatigue had ceased. Making allowance for slight changes in temperature we found that *the current required to balance corresponded as nearly as we could determine, exactly to the current which we estimated would have produced a balance at the instant of turning the torsion head.* The difference was certainly not as great as one part in ten thousand. After several tests of this kind we got rid of the elastic fatigue before beginning observations. If we had neglected the effect of elastic fatigue, we should have got a value for the square of the current too large by one part in one thousand. This corresponds to an error of one part in two thousand for the current itself.

The temperature of the suspending wire was estimated at the time of each balance by means of two thermometers, one inside the case, and one against the brass tube surrounding the wire. (The second thermometer was that used when the torsional constant of the wire was determined.) To facilitate rapidity of work the motion of the movable coil was limited to a small arc by the prongs of a wooden fork (see Fig. 2). Before the

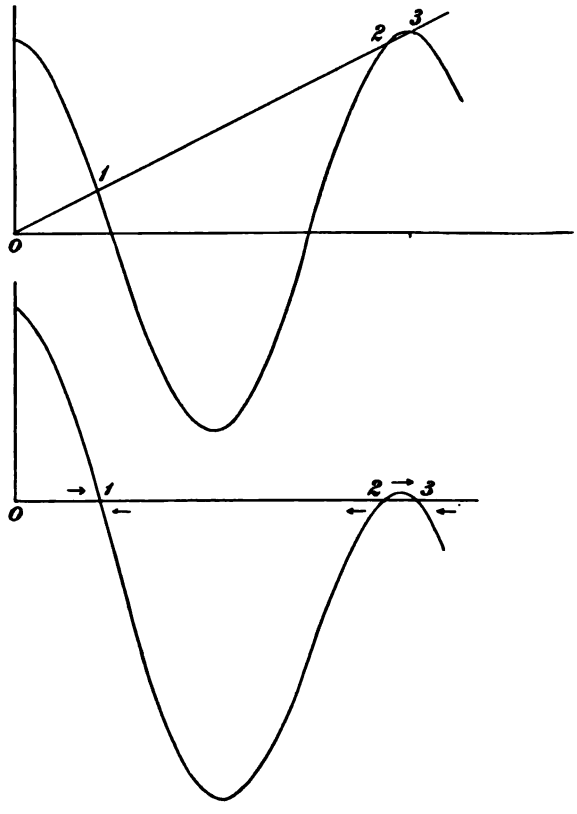


FIG. 4.

fork was used, we found that the suspended coil would on occasion swing through the position of stable equilibrium, on through a position of unstable equilibrium and finally come to rest in another position of stable equilibrium. The torque in the various positions may be represented by the combination of a simple cosine curve with an inclined straight line. The former represents the torque of the electric forces, the latter that of the

suspending wire. In combining the two torques account must be taken of the angle through which the torsion head has been turned. For equilibrium positions must be found in which the two ordinates are numerically equal but opposite in sign. The combined curve has positions of equilibrium wherever the ordinate becomes zero. The positive values of this curve tend to increase the angle, the negative to decrease it. The odd positions correspond to stable and the even to unstable equilibrium. (See Fig. 4.) We used the second position of stable equilibrium and adjusted the current until it corresponded to one complete turn of the torsion head with the movable coil in its zero position. With this arrangement we found that a position of unstable equilibrium was quite near by, and until[1] we had used the wooden fork to stop the coil, it was quite difficult to avoid having the coil turn around while adjusting the current to its right value. The wooden fork completely eliminated this trouble.

TORSIONAL COEFFICIENT OF THE SUSPENDING WIRE.

The torsional coefficient of the phosphor-bronze suspension was investigated very carefully. The wire was soldered at as low a temperature as possible into the two cylinders mentioned before. We did this to avoid any change in the length of the wire used. The larger cylinder fitted as exactly as possible into a hollow cylinder. These cylinders were made from rolled brass bars which we thought more uniform than cast brass. The moment of inertia of the combination was determined very carefully. We weighed them with brass weights which we compared with a set of standard weights, tested by the U. S. Bureau of Weights and Measures and we measured their dimensions by means of a Brown and Sharpe vernier caliper. We tested the graduation of the vernier caliper by means of a dividing engine and found it accurate. As the dimensions of the movable coil were measured by the same instrument, any error due to a faulty unit would disappear. The length and outer radius of the hollow cylinder were in the ratio of $\sqrt{3}:1$ and consequently any slight error in its axis of rotation would produce a minimum disturbing effect on the moment of inertia. To the bottom of the central cylinder a small mirror was fastened with glue. The moment of inertia of both mirror and glue was computed and added to that of the brass. The moment of inertia of the wire was negligibly small. A second hollow brass cylinder as nearly as convenient the duplicate of the first was provided as a check to the first. It was thought that if air holes were present in either cylinder or in both, it would be hardly probable that the same results would be obtained from both. Identical values of the torsional constant of the wire were obtained from both, and it is probable therefore that the real and calculated moments of inertia were the same. Cylinder I was taken as our standard and cylinder II was used as a check only.

The inner smaller cylinder had at its one end a short projection on which the larger hollow cylinders rested. Its moment of inertia is there-

[1] Limb, Compt. rend. 114, 1057, 1892.

fore, considering it as two cylinders of diameters d and D, and length L and l respectively

$$I = \frac{M}{8}\left(\frac{Ld^4 + lD^4}{Ld^2 + lD^2}\right) \qquad (12)$$

DATA FOR THE BRASS CYLINDERS.

A. Inner Cylinder

$M = 47.547$ grms.
$d = 1.270$ cms.
$D = 1.378$ cms.
$L = 4.416$ cms.
$l = .085$ cms.
$I = 9.5599$

B. Hollow Cylinder I

$M = 675\ 950$ grms.
$D = 4\ 992$ cms.
$d = 1.272$ cms.
$L = 4.334$ cms.
$I = 2241.87$

C. Hollow Cylinder II

$M = 674.3474$ grms.
$D = 4.9915$ cms.
$d = 1.272$ cms.
$L = 4.324$ cms.
$I = 2236.7$

D. Moment of inertia of mirror

$I = .465$

Total moment of inertia for Cylinder I $= 2251.89$
for Cylinder II $= 2246.72$

We made a long series of observations on the periods of torsional vibration of the system. To avoid the disturbing influence of the air, we enclosed the system in a long glass vacuum cylinder set in bearings at both ends. The system was set in torsional vibrations by turning the glass cylinder. Within the cylinder was a good thermometer reading directly to 0°.2 C. and a small barometer. During tests the pressure of the air was kept down to about 2.5 cms. of mercury, although a pressure of several times that amount did not affect the period of vibration by more than a negligible amount (for our purposes). We noticed that when the tension of the wire was relieved for some time, a considerable time was necessary before renewed tension brought the wire to the same condition as before. This effect was very slight and produced a variation in the period of vibration of the order of less than one part in ten thousand.

We carefully investigated the temperature coëfficient of the wire, and found in general that the period increased with the temperature at the rate of one part in five thousand per degree Centigrade. From observations with rapidly falling and rising temperatures near zero, we obtained some evidence of a hysteresis effect in the period of vibration. Cold weather did not continue long enough for us to settle the matter definitely. With slowly rising or falling temperatures no such effect was detected.

We also carefully investigated the effect of amplitude of vibration on the period. We here noticed a small effect. The variation of the period between very small amplitudes of vibration and one whole turn from the position of equilibrium was of the order of one part in twelve thousand.

We decided that this was due to friction with the small amount of air in the glass cylinder and molecular friction in the wire itself, and that as these elements would be lacking when in use in the electrodynamometer, the right period for our purposes was the period for small amplitudes. The logarithmic decrement was very small and produced no appreciable effect on the period for small amplitudes.

From these investigations we concluded that the phosphor-bronze wire was suited to use as a suspension in an absolute electrodynamometer. We were well satisfied with its action when in the instrument.

DETERMINATION OF THE PERIOD.

Our method was a slight modification of Kohlrausch's method. On the bottom of the rotating cylinder was a mirror. Around the glass cylinder containing the rotating system was a circular scale subdivided into cms. and mms. at a distance of 20 cms.

The scale was observed through a telescope at a distance of about 150 cms. The instrument was set up in a room, in which there was a Standard Riefler clock, the ticks of which could be heard distinctly. We determined the exact time, when a tick coincided with the passage of the mirror through the middle point of the swing and repeated these observations.

During the interval between two consecutive coincidences a whole number of vibrations was made and dividing this interval by the approximate period we obtain a more accurate value for the latter. Repeating this we can easily obtain an accuracy of .001 second within 200 swings. The method of procedure will become clear from the examples given in the following tables.

The absolute rate of the clock will not come into account in the final determination of the electrochemical equivalent, if the time during which the silver is deposited is also measured with the same clock. We did not, therefore, determine the absolute rate but referred all measurements of time made during the investigation to this Riefler clock as a standard.

Our first object was to determine in how far the elastic fatigue came into account in our measurements. Tables 1 to 3 give three series undertaken at different temperatures. In every case the logarithmic decrement is very small and constant, from which we can conclude that, if there is any elastic fatigue, it is very slight. The amplitude of the vibrations varied considerably in the different tests and decreased to about one-half of its initial value during each series.

TABLE 1.

TIME OF TRANSIT.	NUMBER OF SWINGS.	INTERVAL.	PERIOD.	RIGHT.	LEFT.	AMPLITUDE.	LOGARITHMIC DECREMENT.	TEMPERATURE.
37′ 39″	0			70.9	29.1	41.8		22.6
47 01	47	562	11.957	70.2	29.7	40.5	.000292	22.4
56 59	97	1160	11.958	69.5	30.3	39.2	.000288	22.2
81 18	219	2619	11.9589	68	31.8	36.6	.000285	21.9
130 20	465	5561	11.95912	65.25	34.44	30.81	.000285	22
203 17	831	9938	11 95909	61.9	37.65	24.25	.000283	22
232 59	980	11720	11.95918	60.94	39.0	21.94	.000286	22.1

TABLE 2.

TIME OF TRANSIT.	NUMBER OF SWINGS.	INTERVAL.	PERIOD.	RIGHT.	LEFT.	AMPLITUDE.	LOGARITHMIC DECREMENT.	TEMPERATURE.
38′ 15″	0			62.9	37.4	25.5		16.0
61 10	115	1375	11.957	—	—	—		15.8
76 31	192	2296	11.9580	61.3	38.7	22.6	.000278	16.0
94 51	284	3396	11.957	60.75	39.3	21.45	.000264	16.0
107 48	349	4173	11.9570	60.3	39 7	20.6	.000266	15.5
202 48	825	9864	11.9564	57.8	42.2	15.6	.000259	15.5
211 37	870	10402	11.9563	57.5	42.5	15.0	.000265	15.7
229 21	959	11466	11.9561	57.12	42.88	14.24	.000264	15.7
247 41	1051	12566	11.9562	56.75	43.22	13.53	.000262	15.4

TABLE 3. (Feb. 22.)

TIME OF TRANSIT.	NUMBER OF SWINGS.	INTERVAL.	PERIOD.	RIGHT.	LEFT.	AMPLITUDE.	LOGARITHMIC DECREMENT.	TEMPERATURE.
42′ 07″	0	0		75.7	24.3	51.4		
47 41	28			75.35	24.55	50.8		7
55 26	67			74.9	25.0	49.9	.000291	6.65
58 01	80	954	11.925	74.8	25 15	49.65	.000188	6.65
60 24	92	1097	11.9239	74.52	25.38	49.14	.000212	6.70
62 59	105	1252	11.9238	—	—	—	—	6.70
65 34	118	1407	11.9237	74.2	25.8	48.4	.000221	6.7
70 32	143	1705	11.9237	73.9	26	47.9	.000214	6.65

The second set was taken with the brass cylinder I, the first and third with the brass cylinder II.

It has been mentioned above that the amplitude of the vibrations had a small influence upon the period. The following series of observations shows this very well.

TABLE 4.

TIME OF TRANSIT.	NUMBER OF SWINGS.	INTERVAL.	PERIOD.	TEMPERATURE.
7′ 55″	0	0		18.3
23 27	78	932	11.94872	18.25
31 37	119	1422	11.94958	18.25
37 11	157	1876	11.94905	18.2
62 41	275	3282	11.94907	18.1
70 27	314	3752	11.94905	18.12

The amplitude varied from 55 to 46.5 scale parts.

TABLE 5.

TIME OF TRANSIT.	NUMBER OF SWINGS.	INTERVAL.	PERIOD.	TEMPERATURE.
20′ 00″	0	0		18.4
35 20	77	920	11.9480	18.42
43 18	117	1398	11.94872	18.2
50 52	155	1852	11.9484	18.2
58 50	195	2330	11.94872	18.15
66 36	234	2796	11.94872	18.05
73 58	271	3238	11 9483	18.04
81 56	311	3716	11.94855	18.05

Amplitude varied from 9.5 to 8.8 scale division.

The difference between the period for the two amplitudes is even a little smaller than appears from this on account of the lower average temperature in the second case, which tends to decrease the period.

INFLUENCE OF TEMPERATURE UPON THE PERIOD.

While in the preceding experiments we had simply decided, that the phosphor-bronze wire was well fitted for use in the electrodynamometer and no account had been taken of the thermometer-correction, the following experiments were undertaken to determine the temperature coefficient of the wire and a new thermometer, subdivided into .2° C. was put into the vacuum cylinder.

The following table shows the effect a change of temperature had on the time of vibration of the system. The period was determined in the manner described above. We note here only the final period. These observations are selected out of a large number, made with the same wire. The values in the second column denote the average temperature.

TABLE 6.

DATE.	TEMPERATURE.	PERIOD.
April 4	14.84	11.94083
5	5.91	11.92275
5	9.15	11.9283
9	18.2	11.94905
13	19.32	11 9527

We cannot be perfectly sure about the low temperatures, because it was rather difficult to keep the room at a constant temperature, while we were working in it. Nevertheless it seems apparent that the temperature-coëfficient of the wire increases with rising temperature.

The observations in Table 5 were taken with the big hollow cylinder II and before the wire was fixed for the final determinations. That nothing was changed when the wire was soldered into the brass cylinder, which was to hold the movable coil of the electrodynamometer, is apparent from the following table. These observations were taken with the wire, when ready for use in the electrodynamometer and with ring I.

TABLE 7.

DATE.	TEMPERATURE.	PERIOD.
April 20	20.19°	11 96791
June 30	23.06°	11.97492
"	24.72°	11.9798
July 1	26.89°	11.9851

The first observation was taken before, the last three after, the determinations with the electrodynamometer.

The temperature coëfficient between 15° and 30° is therefore .00022.

The results are plotted in figure 5, I being the curve for the Table 6, and II the curve for Table 7.

EXPERIMENTS WITH THE ELECTRODYNAMOMETER.

In our final experiments we balanced the moment of torsion, due to a complete twist of the wire against the electrodynamic action between the two coils of the electrodynamometer.

The moment of torsion of the wire for a twist of unit angle is

$$w = \frac{4\pi^2 K}{T^2} \quad , \tag{13}$$

where K denotes the moment of inertia of the rotating system and T the time of vibration.

The strength of the field at the center of the stationary coil is

$$H = \frac{4\pi N}{\sqrt{D^2 + L^2}} \; I = h\, I \quad , \tag{14}$$

where N is the number of turns in the stationary coil, D and L its mean diameter and mean length, I the current expressed in C. G. S. units.

The area of the movable coil, multiplied by the number of turns of the same is

$$A = \frac{\pi d^2 n}{4} \quad ; \tag{15}$$

so that the moment of torsion due to the action of the two coils upon each other is, if they are at right angles to each other,

$$w' = h A I^2 = \frac{\pi^2 N n d^2}{\sqrt{D^2 + L^2}} I^2 \quad . \tag{16}$$

The equation of equilibrium is then, the twist being 2π,

$$\frac{\pi^2 N n d^2}{\sqrt{D^2 + L^2}} I^2 = 2\pi w = \frac{8\pi^2 K}{T^2} \quad , \tag{17}$$

or

$$I = \frac{1}{Tr} \sqrt{\frac{2\pi K \sqrt{D^2 + L^2}}{Nn}} \quad , \tag{18}$$

where r is the radius of the movable coil.

Substituting the numerical values in the above formula and taking for the time of vibration of our system 11.9853 at 27°, we obtain as the theoretical value of the *current necessary to balance the torsional moment due to a twist of 2π of the wire at the temperature 27°*

$$I = .98158 \text{ ampere.}$$

To compare this value with the silver depositing current, we made use of Poggendorff's method in the manner described above. (See Fig. 1.)

(1) Balance between the E. M. F. of the standard cells and the difference of potential over the manganin resistance, due to the current necessary to counteract the twisting moment of the wire. As the source of the current through the instrument we used a storage battery with a voltage of about 72 volts. In the following tables we have given the experiments in the order in which they were taken. In the first column is given the arrangement of the coils. In arrangement 2 the current was flowing through the stationary coil in the opposite direction from the direction which it had in the arrangement 1, while the direction of the current through the movable coil remained the same in both arrangements. This was done to eliminate the error due to the lead wires between the two coils. The difference in our results for the balancing current in the two cases is small, but quite apparent. In the second column the number of ohms necessary for a balance with the current through the whole instrument in one direction is given, in the third the same with the current reversed; these two currents correspond to i_1 and i_2 or i_3 and i_4 of equation 11.

The fourth column shows the number of ohms necessary to balance the E. M. F. of the two Carhart-Clark cells; the following, the E. M. F. of the cells, supposing the value at 24° to be 1.435 volts. The following two columns contain the temperatures of the electrodynamometer (t^d) and the cells (t_c) respectively. The value of the manganin resistance was taken as 4.626 ohms. The current I', calculated under these suppositions and reduced to 27° of the electrodynamometer, is given in the last column and shows no larger discrepancy than one in ten thousand for the same arrangement.

TABLE 8.

ARRANGEMENT.	R_a	R_b	R_1	E. M. F.	t_c	t_c	I'
1	7901.5	7773	4944.1	2.86878	26.35	24.95	.98284
2	7897	7765.7	4944.1	2.86878	27.8	24.95	.98246
2	7896	7764.6	4942 9	2.86795	28.09	25.6	.98236
1	7902	7773	4942 8	2.86744	26.68	26	.98274
2	7898.6	7770.7	4943	2.86744	26.95	26	.98242
2	7902.6	7773.4	4944.9	2.86808	26.03	25.5	.98242
1	7904	7774.1	4944.85	2.86795	27.39	25.6	.98284

Since we were not sure of the temperature-coëfficient of the Carhart-Clark cells at the high temperatures, at which they were used, a separate experiment was made to find the variation of their E. M. F. with temperature. A Clark cell (United States standard) was always kept at a constant temperature and the temperature of the experimental cell changed. No observations were taken immediately after a change, but the temperature had been practically constant for several hours. Supposing the E. M. F. of a Carhart-Clark cell to be 1.435 volts at 24° C. we found the following values:

TABLE 9.

TEMP.	E. M. F.
22	1.43628
23	1.43564
24	1.43500
25	1.43436
26	1.43372

These values have been used in table 8.

Instead of calculating I' we may correct the resistances R_a and R_b to the values they would have had, if the electrodynamometer had been at 27°, using the temperature-coefficient, found for the wire and correct R_1 to 24° of the cells.

TABLE 10.

ARRANGEMENT.	R_a	R_b	R_1
1	7900.4	7771.9	4946.3
2	7898.4	7767.1	4946.3
2	7897.8	7766.4	4946.4
1	7901.5	7772.5	4947.2
2	7898.5	7770.6	4947.4
2	7901.0	7771.8	4948.2
1	7904.5	7774.6	4948.3

Calling R'_a and R'_b the resistances found with the arrangement 1, R''_a and R''_b those with arrangement 2, we find as the resistance necessary for a balance, if there were no influence of the earth's magnetism or asymmetry in the instrument,

$$R_2 = \sqrt{R'_a R'_b R''_a R''_b}.$$

The values of R_1 and R_2, calculated from the numerical results, are

$$R_1 = 4947.2$$
$$R_2 = 7835.5$$

and therefore

$$I = 1.58383 \frac{E}{R} \text{ amp.} \quad (1a)$$

(2) Balance between the E. M. F. of the standard cells and the difference of potential due to the silver depositing current.

The method of procedure during these experiments was the following. As in the preceding we used the storage battery, having about 70 ohms in series with the two voltameters. Before sending the current through the voltameters it was allowed to flow through the resistance, in order to heat it up; a preliminary balance was taken. Then, by means of a switch, the silver voltameters were thrown in series with the rest of the circuit. By means of this precaution we obtained a very constant current and the small changes taking place were so regular that there was no difficulty in taking an average of the resistances necessary for a balance during the hour the current was running. In taking the average, we were careful to allow for the time-interval between two consecutive balances. Out of

a number of observations we have selected four in which the changes in the resistances were small, as an average about 20 ohms in 7500 ohms during the hour, and in which the temperature of the standard cells was near 24°. The change of the E. M. F. of the standard cells never exceeded two in fifty thousand during the whole hour.

The rate of the clock was repeatedly compared with the rate of the Riefler clock and it was found that one hour was equal to 3601.3 seconds.

In the first two of these determinations two standard Carhart-Clark cells were used, whose E. M. F. was a little smaller than that of the cells used in the rest of the experiments. The ratio was determined to be 1.00243. In the following table R_2' is the average resistance necessary to balance the current and R_1' the resistance to balance the E. M. F. of the cells. In the fourth column are given the corrected values of R_1', *i. e.*, the resistances that would balance the cells if they were at 24° C.; in the following the temperature of the cells and in the last the electrochemical equivalent worked out under the supposition that the values of E (2.870 volts) and R (4.626 ohms) are correct.

TABLE 11.

DATE	R_2'	R_1'	R_1' CORR.	T	SILVER DEPOSIT.	z
June 4	7622.15	4830.71	4842.6	24.14	2.6211 grms.	1.11795
19	7529.25	4911.05	4919.2	22.3	3.82301	1.11789
27	7530.69	4938.79	4938.4	23.82	3.8094	1.11807
30	7664.86	4943.75	4944.9	24.5	3.8718	1.11796

In the first case the duration of the experiment was only forty minutes; in the rest, sixty minutes.

Taking the mean of R_1', R_2' and q, the silver deposited in one hour, we obtain

$$R_1 = 4911.3 \text{ ohms}$$
$$R_2' = 7586.7 \text{ ohms}$$
$$q = 3.8589 \text{ grms.}$$

Substituting these values and those obtained above for I_1 R_1 and R_2 in our formula (5)

$$z = \frac{q}{It} \frac{R_1'}{R_1} \frac{R_2}{R_2'}$$

we find as the electro-chemical equivalent of silver

$$z = \frac{3.8589 \times 4911.3 \times 7835.5}{.98158 \times 3601.3 \times 4947.2 \times 7586.7} \text{ grms.} = .0011192 + \text{grms.}$$

This is about ½ % larger than the accepted value. It agrees with the average value of Pellat and Potier, and with the value Kahle found when

using an old solution of silver nitrate. Kahle gives for this in his preliminary report (Zeitschr. f. Instr. XVII, 144, 1897) a value almost identical with ours, 1.1193 mgrm. Our experiments with the silver voltameters were taken with a solution that had been used three or four times before.

Moreover it is of interest to note that the discrepancy between Rowland and Griffith's value of the mechanical equivalent of heat disappears, when we use as the unit of current one about .11% larger than the ampere.

A recalculation of the E. M. F. of the Clark standard cell from the new value for the electro-chemical equivalent of silver gives, if we take as the old value 1.4342 volts, as determined by Glazebrook with the electrochemical equivalent of .001118 grm.

$$E = 1.4327 \text{ volts at } 15°C.$$

Kahle's value is 1.4328 volts.

Kahle has found that the E. M. F. of a standard cell is more constant and gives better results than a silver voltameter. With our apparatus we can easily determine the E. M. F. of such a cell by direct comparison and an accurate determination of the manganin-resistance.

It is intended to continue the experiments with special reference to the E. M. F. of the Clark cell.

The following reports and papers were read before joint session of Sections A and B, and are printed in Section A:

REPORT ON THE RECENT PROGRESS IN THE DYNAMICS OF SOLIDS AND FLUIDS. By Dr. ERNEST W. BROWN, Christ's College, Cambridge, Professor of Applied Mathematics, Haverford College, Pa.

THE MASS AND MOMENTS OF INERTIA OF THE EARTH'S ATMOSPHERE. By Prof. R. S. WOODWARD, Columbia University, New York City.

THE NEW FORMS OF APPARATUS FOR MEASURING THE ACCELERATION OF GRAVITY. By Prof. R. S. WOODWARD, Columbia University.

THE GRAVITATION CONSTANT AND THE MEAN DENSITY OF THE EARTH. By Prof. R. S. WOODWARD, Columbia University.

The following papers were read before joint session of Sections B and D and are printed in Section D:

HIGH SPEED INFLUENCE MACHINES. By CHARLES F. WARNER, Cambridge, Mass.

PROPOSED METHODS OF DETERMINING THE FREQUENCY OF ALTERNATING CURRENTS. By CARL KINSLEY, Falls Church, Va.

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CHEMISTRY.

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ADDRESS

BY

EDGAR F. SMITH,

VICE PRESIDENT, AND CHAIRMAN OF SECTION C.

THE ELECTRIC CURRENT IN ORGANIC CHEMISTRY.

It is now almost a century since Volta, the Italian physicist, wrote the following words:

"I have made experiments, showing the transmission of the electric fluid . . . I have applied different metals to all sorts of animal bodies . . . even to such substances as paper, leather, linen (well saturated with water), as well as to water itself . . . . The metals are not only conductors of electricity, but they also excite it, and this is a grand discovery!"

Yes, it was, indeed, a "grand discovery," for it led to further investigation with the final demonstration that animal and metal electricity were identical. What is more, it unquestionably opened the way to the construction of the battery bearing the name of this honored investigator, who, however, little dreamed of the splendid achievements which were to follow the introduction into chemistry of the form of energy, made so readily accessible by his battery and its numerous subsequent modifications; for history tells us that he failed to observe the decomposition which arose upon immersing the terminals of one of his cells into water. That was to remain for the keener vision of Nicholson and Carlisle.

The chemical phenomena, exhibiting themselves constantly to Volta while experimenting with his battery, were to him absolutely devoid of interest, yet they continued to crowd to the front

and eventually attracted the attention of a brilliant coterie of investigators, whose discoveries could never have come in their most shadowy forms to Volta in his wildest dreams! In evidence of this I need merely mention the electrolytic decomposition of water, just alluded to, by Nicholson and Carlisle; the deposition of the metals lead and silver (probably the beginning of electro-chemical analysis); the remarkably mysterious and quiet passage of oxygen from the end of the silver terminal over to the zinc terminal where it appeared as gas—all observed by Cruikshank; the discovery by Davy that the current set free hydrogen and oxygen from water in the proportion in which they united to form the parent substance; the conclusion reached by Ritter that reduction invariably took place at one pole while oxidation occurred at the other; the evolution of Grothuss' theory in regard to the decomposition of liquids generally by the current—a theory highly suggestive in various directions (thus, the esterification theory of Williamson is based upon the same idea of alternate decompositions and reformations); the magnificent conquests made by Davy, culminating in the isolation of the interesting and valuable alkali metals, sodium and potassium; giving to the chemical world not only two new elements, but also inciting students the world over to research in wide reaching fields —a discovery pronounced by Ostwald to be "the close of the first act of the electrochemical drama," and yet what wonderful progress had been made through the application of the new agent! But richer discoveries were to follow. Recall the labors of Berzelius and Hisinger upon which was founded a theory now almost forgotten; and in its day a retarder, in some measure, of the proper understanding of simple chemical combinations. But it was helpful and contributed at least a mass of facts, applied later successfully in the solution of various chemical problems. And as we advance I must not fail to mention Becquerel, de la Rive and Pfaff, who contributed much of lasting value during the controversy waged by the adherents of opposing schools in regard to the origin of the current, in the midst of whom appeared the immortal Faraday, fixing the relation between the electrical magnitudes and the combining numbers of chemistry; really taking the initial steps in the foundation of the quantitative period of electrochemistry. I must not, however, dwell too long upon the many observations made by him. Let suffice the mere mention of his identification of machine and Volta electricity; his declaration concerning electrochemical

decomposition that "the effect is produced by an internal corpuscular action, exerted according to the direction of the electric current, and that it is due to a force either superadded to or giving direction to the ordinary chemical affinity of the bodies present;" his suggestion, after wrestling with the nomenclature of that day prevalent in electrochemistry, of the terms anode, kathode, electrode, ion, etc., etc.; his efforts to measure electricity when he wrote "that the decomposing action of the current is constant for a constant quantity of electricity," and following this thought constructed an instrument which, "being interposed in the course of the current, used in any particular experiment, should serve at pleasure either as a comparative standard of effect, or as a positive measurer of this subtile agent." Again, having repeated all his earlier experiments with the most varying conditions, he wrote "variations in the intensity . . . . produced no difference as to the equal action of large and small electrodes;" and when he had completed his volta-electrometers they were so arranged that "after the current had passed through one it should divide into two parts; these after traversing each one of the remaining instruments should reunite. The sum of decomposition in the two latter vessels was always equal to the decomposition in the former vessel . . . . exactly the same quantity of water was decomposed in all the solutions by the same quantity of electricity." When the current acted upon caustic soda or potash, magnesium sulphate or sodium sulphate, just as much hydrogen and oxygen were evolved from them as from the diluted sulphuric acid with which they had been compared. And his final declaration in regard to water "that when subjected to the influence of the electric current, a quantity of it is decomposed exactly proportional to the quantity of electricity which had passed." Nor can I refrain from a brief allusion to the electrolysis of molten salts, also conducted by Faraday. You all remember the experiment he performed with lead chloride. The weighed platinum wire kathode was introduced into the molten salt; "a button of alloy could be observed gradually forming and increasing in size." In time the experiment was interrupted, when it was discovered that the "positive electrode had lost just as much lead as the negative had gained, ... the equivalent number, by comparison with the result in the volta-electrometer, being 103.5." Some one has said and I think we all will echo the sentiment, that "the data communicated by Faraday as the result of his years of observation of the action of the electric current upon chemical

compounds, are among the most important in the history of electrochemistry." They form the basis of all the quantitative laws which have been developed in this special field. They merit place side by side with the law of combining weights.

Equally important, perhaps, both from a theoretical and practical standpoint have been the results recently won by the introduction of the current into the field of pure chemistry through the persistent efforts of untiring investigators. Mark how, in the diffusion of solids in their solvents, van't Hoff perceived a similarity to the expansion of gases; how he succeeded in measuring that diffusive, expansive force, bearing the name osmotic pressure; then note the astonishing consequences of this discovery. The simple relation between gas volume, attending pressure and temperature had led Avogadro to promulgate his hypothesis in regard to molecules — a thought considered by all of us as a fundamental in our science. This hypothesis van't Hoff made bold to apply to solutions and there resulted a theory for solutions analogous to that previously wrought out for gases. To-day molecular weight determinations are made almost daily in our research laboratories as an indirect consequence of this deduction. Indeed, the laws governing osmotic pressure, when applied in the most varied forms, remained satisfactory except with solutions of salts, bases and acids. Their osmotic pressure was far beyond what could be expected from the theory of van't Hoff. How was this to be explained? The answer came from Arrhenius (1887): the departure from the law is due to dissociation, and most astonishing of all is that the dissociation of bodies in their solvents occurs only in the case of those which conduct the electric current, *i. e.* with electrolytes. Compounds not dissociated in aqueous solution — which adhere to the law of osmotic pressure — do not conduct the electric current. There remained but a step to the assumption that the products of dissociation, in aqueous solution, were identical with the substances termed ions, which in electrolysis appear at the electrodes. A logical query, consequent upon this declaration, was:

Are, for example, free potassium and free chlorine present in an aqueous solution of potassium chloride? Let me read you Ostwald's well-known reply to this question:

"What actually exists in the solution are single potassium atoms with enormous electrical charges. We do not know what these charges are in reality, but this we know — that the chemical

properties of substances are greatly altered by electrical charges. On the other hand what we know as free potassium is a solid substance whose molecules consist of potassium atoms not charged with electricity at all . . . . As soon as the potassium atoms in solution lose their charge, as they do, for example, when led by an electric current to a platinum wire, where they can give up their electricity, potassium with its ordinary properties is at once produced."

A further consequence of osmosis and that of free ions in solution is Nernst's theory in regard to the electric current in the voltaic cell — the little instrument sent into the world a century ago, from which have come such epoch-making results, so that one cannot help but be profoundly impressed with the time-honored words — "despise not the day of small things."

However, it is not alone in the field of pure chemistry that helpful and interesting consequences of the introduction of the "subtile agent" are found. The patient, careful analyst, as he struggles to unravel the problems Nature has placed before him, will ever hold sacred the name and memory of our own Dr. Gibbs who, in 1865, first called the attention of the chemical world to that simple yet beautiful and extremely satisfactory determination of copper in the electrolytic way, which proved the incentive to further study in this direction; so that at present twenty-one metals lend themselves to electrolytic determination from solutions of the most varied character, and in the same manner at least one hundred and twenty separations have been made, all of which for accuracy, neatness and rapidity leave little to be desired.

In the commercial manufacture of potassium chlorate, potassium persulphate, caustic, iodoform, etc., etc., those who seek to apply the principles of chemistry in the establishment of industries of national import, have found in the current a most fruitful aid, the full consequences of which are yet untold.

But it was not to any one of these I especially desired to direct your attention. As indicated by my subject I had in mind another thought and, craving your further indulgence, I shall at once proceed to a new chapter in this historic résumé of electrochemistry.

You are all familiar with the classic experiment made fifty years ago (1848) by Kolbe. I refer to the electrolysis of concentrated solutions of potassium acetate and valerate. You remember that this was the first investigation of its kind in the domain of organic chemistry. The results were most interesting and were thought

to have shed a new light upon and given deeper insight into the constitution of compounds. In the case of potassium acetate the course of the decomposition was considered as being fully represented by the resulting products:

$$\overbrace{CH_3.CO_2}^{+}\overbrace{K}^{-} \quad \overbrace{H\,OH} \quad = \quad \overbrace{CH_3}^{+} \quad \overbrace{CO^2} \quad \overbrace{KOH}^{-} + H$$

$$CH_3.CO_2\,K + H\,OH = CH_3 + CO_2 + KOH \quad H$$

It was held to be a very simple decomposition.

Kekulé in 1864 gave to the world facts gathered by him in the electrolysis of dibasic acids, *e. g.*, succinic, fumaric, maleïc, and brom-maleïc acids. The first gave hydrogen and ethylene, together with carbon dioxide; the two isomeric acids gave acetylene, while brom-maleïc acid was thought to have yielded brom-acetylene. In 1866 Brester subjected various aliphatic bodies to electrolysis, but obtained nothing new. Aarland next followed with his study of the three isomerides—citraconic, itaconic and mesaconic acids, or the allylene dicarboxylic acids as they may also be termed. Aarland expected that when these three bodies were subjected to electrolysis they would yield three isomeric allylenes. He also purposed testing the suggestion of Carstanjen that six isomeric allylenes really existed. Without entering into details I may say that itaconic acid was electrolyzed with ease; its gaseous products were allylene and carbon dioxide. The residue contained acrylic and mesaconic acids. The resulting allylene was completely absorbed by bromine, but not by an ammoniacal silver nitrate solution. Citraconic acid yielded allylene, containing hydrogen exchangeable for metal and acrylic and mesaconic acids. Similar products were discovered in the electrolysis of potassium mesaconate.

Mesaconic and citraconic acids bearing the same relation to one another as fumaric acid sustains to maleïc acid, of course yielded allylene, $CH_3C=CH$, whereas itaconic acid was resolved by the current into symmetrical allene, $CH_2=C=CH_2$.

As we search farther we shall learn that, under the influence of the electric current, butyric acid was observed to give hexane, valeric acid, octane, caproic acid, decane, etc. Oxalic acid was completely changed to carbon dioxide and water. Malonic acid was resolved into the same components, but the dissociation proceeded less rapidly. Upon electrolyzing an alkaline solution of sodium

succinate, Bourgoin observed ethylene and acetylene in addition to the customary gas products. Free malic acid was decomposed very slowly; its alkali salts yielded carbon dioxide, monoxide, oxygen, aldehyde and acetic acid. Tartaric acid was broken down into acetic acid. Lactic and sarcolactic acids, in neutral solution, gave acetaldehyde, while in alkaline solution, owing to the condensing power of the alkali, aldol or crotonaldehyde appeared. Glyceric acid was resolved into formaldehyde, carbon dioxide, monoxide and water. In the case of the electrolysis of β-hydroxy-acids the rule seems to have been oxidation; the possibility of condensation, however, was not excluded. Formaldehyde was obtained in very appreciable quantities in the electrolysis of a concentrated solution of glycollic acid, whereas in dilute solution the products were carbon dioxide, carbon monoxide and very little formaldehyde.

Let us pause a few moments and consider how these decompositions have possibly occurred. Kolbe considered the course of the action as very evident. The equation previously given represents his idea, "the acetic acid in the field of the galvanic action is oxidized by the oxygen so that it is resolved into carbon dioxide and methyl, both of which appear at the positive pole, while only hydrogen is formed at the negative electrode."

Kolbe, Kekulé and others regarded the decomposition of the organic acid as a secondary reaction. The oxygen, arising in the water-decomposition, they considered exerted an oxidizing effect upon the acid. Bourgoin's researches upon the electrolysis of organic acids were very extensive, and it was the opinion of this investigator that the most important phase in their electrolysis was the production of intermediate anhydrides of the acids. These, Bourgoin thought, then parted with oxygen, and secondary products were formed. In this latter class he included the passage of the anhydride into the acid by water absorption, as well as the oxidation of the acids by the oxygen released from the acid.

These ideas represented by symbols would appear as follows:

$$1.\quad \begin{matrix} CH_3.COO \\ CH_3.COO \end{matrix}\Big|\, K_2 \qquad 2.\quad \begin{matrix} CH_3\cdot CO \\ CH_3\cdot CO \end{matrix}\Big\rangle O + O \Big|\, + K_2$$

or

$$3.\quad \begin{matrix} CH_3 \\ | \\ CH_3 \end{matrix} + CO_2 + O.$$

But, we continue to ask, are an anhydride and oxygen formed before the hydrocarbon is produced? If so, does the oxygen then decompose the anhydride with the formation of the hydrocarbon and carbon dioxide? We are not alone in this inquiry, but Löb has endeavored to find an answer in the experimental way. To do this, he electrolyzed phthalic acid, dissolved in alcohol (a little sulphuric acid was added to increase the conductivity) with a feeble current. The latter was allowed to act for several hours and there resulted an almost quantitative yield of phthalic anhydride. It is not surprising that this chemist inclines to the view of an intermediate anhydride formation, but he wisely says that this is only demonstrable with acids, whose anhydrides are very stable.

In dealing with the electrolysis of free acids we can scarcely assert that breaking-down occurs, because the acid reappears as such at the positive electrode. Graphically represented we should have:

(1)
$$2CH_3.COOH = 2CH_3.COO + H_2$$
$$2CH_3.COO + H_2O = 2CH_3.COOH + O$$

or

$$2CH_3.COOH = 2CH_3.COO + H_2$$
$$2CH_3.COO = (CH_3.CO)_2.O + O$$
$$(CH_3.CO)_2O + H_2O = 2CH_3.COOH.$$

The first equation shows that oxygen comes from the water, while in the second it has its origin in the intermediate anhydride formation. But let us follow Löb and with him substitute hydrogen sulphide for water, then the equations just written would appear thus:

$$2CH_3.COOH = 2CH_3.COO + H_2$$
$$2CH_3.COO + H_2S = 2CH_3.COOH + S$$

or

$$2CH_3.COOH = 2CH_3.COO + H_2$$
$$2CH_3.COO = (CH_3.CO)_2O + O$$
$$\begin{matrix} CH_3.CO \\ CH_3.CO \end{matrix} > O + H_2S = CH_3.COOH + CH_3.COSH.$$

What do these teach us?

1. If an intermediate anhydride is not formed then the original acid will be regenerated and sulphur will separate.

2. If the anhydride is produced an evolution of oxygen will take place, and a molecule of a thio-acid will be obtained for every molecule of the ordinary acid.

An experiment was performed to test these ideas. Very pure acetic acid was reduced to a low temperature and in this condition saturated with hydrogen sulphide. It was electrolyzed, the gas being still conducted into the solution. Not a particle of sulphur separated at the anode, and the solution contained thio-acetic acid.

Two experiments, therefore, indicated the formation of an intermediate anhydride. However, other views are prevalent. Thus, Schall entertains the opinion that a peroxide is first produced in the electrolytic decompositions of the class we are considering. He electrolyzed the alkali salts of dithio-acids and obtained persulphides of the acids:

$$2CH_3.CSS - = \begin{matrix} CH_3.CSS \\ | \\ CH_3.CSS \end{matrix},$$

and then the action ceased. It may be because the persulphides are so much more stable than the corresponding peroxides. Schall apparently has been confirmed and indeed antedated in his views, for Bunge in 1870 subjected thio-acetic acid and thio-benzoic acid to electrolysis and obtained acetyl disulphide and benzoyl disulphide, "facts which," the author remarks, "justify the conclusion that the compound group separating in the electrolysis of the thio-acids, at the anode, is liberated as a complex."

Hamonet conducted an extensive series of experiments with the alkali salts of the fatty acids. Inasmuch as he worked upon a much larger scale than his predecessors his conclusions are entitled to thoughtful consideration. They are in substance:

1. The equation, expressing the decomposition of salts of the fatty acids, as introduced by Kolbe, cannot be regarded as correct. Frequently, paraffin hydrocarbons are not obtained, or at least in very small amounts.

2. The olefines predominate in this decomposition.

3. An alcohol with n-carbon atoms is invariably formed if the acid contains $n + 1C$, and its structure is not always what we might expect.

So much for the acids. Plainly this particular line of inquiry needs and deserves further attention from the investigator.

The changes induced in organic substances by the electric current are, as a rule, so complex that their study is surrounded by vastly more and greater difficulties than obtain in the electrolysis of inorganic bodies. Von Miller, recognizing this feature, has adopted means of continuously exposing and withdrawing electrolyzed products from the field of action. In this manner he has striven to obtain a more complete picture of what actually occurs under the influence of the current. In the electrolysis of a mixture of potassium acetate and potassium glycollate the product was ethyl alcohol:

$$\begin{matrix} CH_3.COOK \\ CH_2.OH.COOK \end{matrix} = K_2 + 2CO_2 + CH_3.CH_2OH$$

True, the quantity of alcohol was not great, but its formation was certainly very significant. Through the instrumentality of the current even iodine and the nitro-group have been introduced into organic derivatives. Thus, the electrolysis of potassium propionate, in the presence of potassium iodide, yielded ethyl iodide, and that of sodium propionate and sodium nitrite, nitroethane.

Brown and Walker, keeping in view the fact that Kolbe had synthesized hydrocarbons by electrolyzing fatty acids and the additional knowledge that ester groups are electrolytically inactive (first observed by Guthrie), effected some most interesting electrosyntheses, an outline of which is all that I can now give you. They reasoned in this way: If the mono-esters of dicarboxylic acids are electrolyzed, carbon dioxide will be eliminated, and a diester of some higher dicarboxylic acid will be formed:

$$2\,C_2H_5.OOC.CH_2.COOK = C_2H_5.OOC{\cdot}CH_2.CH_2COOC_2H_5 + 2CO_2.$$

The experiment was performed with potassium ethyl malonate. The concentrated solution of this salt was introduced into a platinum crucible, which served as the kathode. A heavy platinum wire, in spiral form, constituted the anode. The current density approached three hundred amperes per square decimeter. A yield of sixty per cent of the theoretically required succinic ester was obtained. They further synthesized adipic acid and sebasic acid. In all of these syntheses secondary changes were noticed, so that it is in no wise surprising that the reaction of Brown and Walker was not entirely successful with the ester salts of unsaturated dicarboxylic acids, in which an ethylene union is present.

The ester potassium salts of phthalic acid, of benzyl malonic acid and the salts of oxyacids, such as malic, proved unavailable for synthetic purposes.

I must not omit mention of a rather interesting result, recently communicated by v. Miller and Hofer. As observed, these chemists have entered quite largely into the study of the electrolysis of what may be termed mixtures. They have applied the new method to tricarballylic acid with the view of determining constitution. This acid, as you are all aware, is tribasic:

$$\begin{array}{l} CH_2.COOH \\ | \\ CH.\ COOH \\ | \\ CH_2.COOH \end{array}$$

From it we can obtain two different mono-esters, two di-esters, and one tri-ester. Quoting von Miller and Hofer freely I shall say that there is no ordinary chemical procedure which will determine, for example, the position of the ester groups in the diesters. So these investigators set about and saponified the triester one-third; they then mixed the potassium salt of the resulting diester with potassium acetate and subjected the mixture to electrolysis. Large quantities of ethyl succinic ester were formed, a proof of the presence of the unsymmetrical diester in the saponification product, for the symmetrical diester would have yielded methyl glutaric ester. The authors went farther. They had obtained a diester acid, constitution unknown, while preparing the triester. The potassium salt of this diester acid they mixed with potassium acetate and electrolyzed as before. The product again gave ethyl succinic ester, showing that the original body was the unsymmetrical diester. These are the first examples of electrosyntheses being used to determine constitution, which could not be arrived at in any other way.

Mulliken has also brought to light a new class of electrosyntheses. He has directed his investigation mainly to bodies containing a $-CH_2-$ or $-CH-$ group, in union with more negative groups. We know that such substances contain hydrogen, which can be replaced by metals. Their sodium salts in alcoholic solution conduct themselves like electrolytes; they break down into sodium and a complex anion—the carbonaceous residue. "The products obtained in all the electrosyntheses, thus far made, are the same one would

expect to be formed by the action of iodine on the sodium or silver compounds of the substances electrolyzed:

$$2C_7H_{11}O_4Na + 2\,I = 2NaI + C_2H_2(CO_2C_2H_5)_4$$

Sodium diethyl malonate — Ethyl ethane-tetracarboxylate

$$2C_7H_{11}O_4Na\ (\text{electrolyzed}) = 2Na + C_2H_2\,(CO_2C_2H_5)_4.$$"

Weems has continued this line of investigation and from his results has felt justified in concluding that the electrosyntheses effected from malonic ester, methin-tricarboxylic ester, acetylacetone and acetoacetic ester are best explained as the result of the direct union of the anions. "Pairing of anions has been shown to be of particularly common occurrence in the electrolysis of sodium compounds of derivatives of malonic ether." Such pairing has been observed in certain examples of the aceto-acetic ester type, but it can scarcely be said to be general. Pairing of anions does not take place in the electrolysis of compounds like acetamide, benzamide, succinimide and phthalimide. The same holds with their sodium and mercury salts.

A very recent synthesis is that of dinitrohexane, a result of the electrolysis of potassium nitroisopropane.

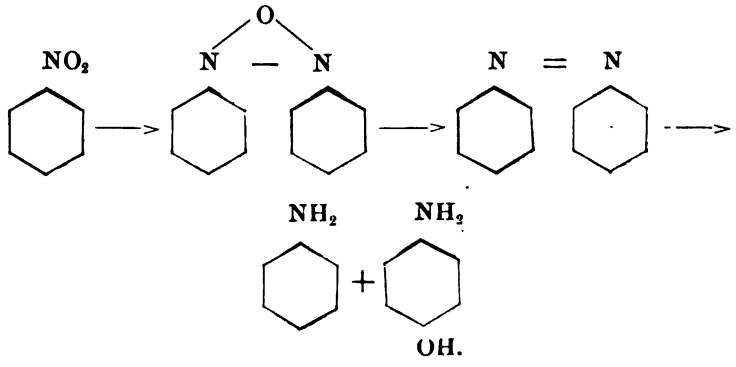
Let us take a cursory glance at the deportment of the alcohols, when exposed to the current. Methyl and ethyl alcohol have been electrolyzed with electrodes of platinum and electrodes of other substances, with and without diaphragms. The results have been of the most varied character. In aqueous sulphuric acid solution methyl alcohol gave methyl formate, acetic acid, and methyl acetate. Formic aldehyde was not observed. Methane was produced when the electrolyzed solution contained potassium carbonate. Ethyl alcohol, in sulphuric acid solution, gave acetic acid, formic ester and ethyl sulphuric acid. Chlorinated acetic acids were obtained when hydrochloric acid was substituted for sulphuric acid. Glycol, in sulphuric acid, gave trioxymethylene, glyceric acid, formic acid and an isomeric glucose. Glycerol yielded formic and acetic acids, together with glyceraldehyde. Chloral hydrate, in presence of sulphuric acid, with a diaphragmed cell yielded chlorine and acetaldehyde. The hexahydric alcohols and sugars gave products similar to those obtained from glycerol. Thus, grape sugar was resolved into carbon dioxide, carbon monoxide, trioxymethylene, and saccharic acid.

Now turn with me to the aromatic series. It is here that stable

nitro-derivatives flourish, so to speak, and in the reducing side of the current chemists possess a means of attacking these bodies most energetically. Let us take nitrobenzene as the particular subject for consideration. That this yields azobenzene, when it is reduced in an alkaline solution, and aniline in an acid solution, are well established facts. Does this hold true when the electric current is the agent of reduction?

The reply to this inquiry divides itself. In an alkaline solution azobenzene and hydrazobenzene are formed. In an acid solution the picture is different from that which is generally supposed to represent the course of the reduction, for Gattermann found p-amidophenol. The explanation given for the presence of this body is that when the current reduces nitrobenzene the first product is phenylhydroxylamine, $C_6H_5NH.OH$, and that the sulphuric acid present rearranges this compound into p-amidophenol. Aniline was invariably found with the latter body. This led Elbs to subject the process to a more careful and critical study. Gattermann considered that the sulphuric acid figures very seriously in the reduction. Indeed, he thought that it played a triple role, as it conducted the current, occasioned the molecular rearrangement, and served as the solvent. The question, which naturally suggested itself to the inquiring mind was, could the quantity of this reagent in any manner modify the course of the reaction? Could it affect the yield of the several substances? So it was proposed to substitute acetic acid of varying concentration. The yield grew larger but that of the aniline increased with the increased amount of p-amidophenol. Evidently, then, aniline could not be considered in this reduction, by the current, as a secondary, but rather as a normal product. A kathode of lead was substituted for that of platinum, and strangely enough the quantity of aniline became greater. Was it possible that the aniline was due to a secondary reduction of the p-amidophenol? Or are the lead electrodes responsible for the direct reduction of nitrobenzene to aniline? Experiments, made to test the first point, demonstrated that p-amidophenol, when operated upon with the conditions as before, was not reduced to aniline. After much search it was found that p-amidophenol and aniline were produced in equal proportions with platinum electrodes, by prolonging the period of action. Therefore, it must have been the spongy lead which effected the reduction of the nitrobenzene to aniline. Gattermann

remained firm in his view that nitrobenzene was first reduced to phenylhydroxylamine, which in turn became p-amidophenol, and contended that the latter ought to be obtained entirely free from aniline. However, azoxybenzene had also been detected in the reduction mass, and naturally the thought followed that possibly the p-amidophenol was derived from it. The equation which I add will explain this reasoning:



Observe, by adopting this idea of the reduction we can plainly comprehend the presence of both aniline and p-amidophenol. Experiment alone could decide whether this was the true course. Accordingly nitrobenzene and azoxybenzene were electrolyzed under similar conditions. The first gave an abundant yield of p-amidophenol, while scarcely any of it was obtained from the azoxybenzene. The evidence, therefore, strongly favored Gattermann's original conception, viz.: that the p-amidophenol had its origin in the phenylhydroxylamine—the intermediate product. The question continued to interest chemists to such a degree that Löb suspended nitrobenzene in fuming hydrochloric acid or in alcoholic hydrochloric acid, or in mixtures of hydrochloric acid and acetic acid, and reduced it electrolytically. The products were p- and o-chloranilines. The corresponding bromanilines were obtained when hydrobromic acid was used. Löb and Gattermann concluded then that the reduction was correctly represented in the following equations:

1. $C_6H_5NO_2 + 4H + HCl = C_6H_5NHCl + 2H_2O$,
2. $C_6H_5NHCl = C_6H_4Cl\,NH_2$

o-and p-aniline.

While with Elbs the course of reduction would be represented in this manner:

1. $C_6H_5NO_2 + H_2 = C_6H_5 - N(O) - N - C_6H_5 + H_2O.$

2. $C_6H_5 - N(O) - N.C_6H_5 = C_6H_5N = N.C_6H_4.OH$

3. $C_6H_5{-}N = N.C_6H_4OH + 2HCl = C_6H_5NHCl + HClNC_6H_4OH$

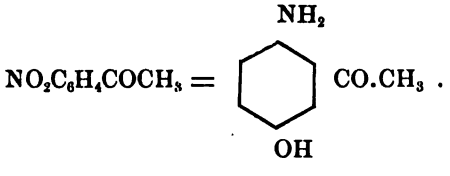
$$= \frac{C_6H_4ClNH}{a} + \frac{C_6H_3(OH)Cl.NH_2}{b}$$

A second body (b) chloramidophenol should be produced if the view of Elbs was correct. It was, however, at no time discovered in the reaction mass, thus affording additional evidence of the correctness of the first idea. It is not necessary for me to submit all the evidence pointing to the presence of the extremely unstable intermediate compound C_6H_5NHCl, which rearranged itself into the chloranilines. Enough to know that it is very convincing and has been further corroborated by the isolation of the body $C_6H_5NCl.CO.CH_3$, observed by Bender.

Gattermann, seeking further support for his views, introduced benzaldehyde into the reduction mixture at the very outstart. He obtained benzylidene hydroxylamine, a condensation product previously prepared by Bamberger upon mixing benzaldehyde and phenylhydroxylamine:

$$C_6H_5NH.OH + C_6H_5.CHO = C_6H_5.N(O) - CHC_6H_5 + H_2O.$$

Further, o-nitrotoluene and benzaldehyde gave benzylidene-o-tolyl hydroxylamine, and m- and p-nitrotoluenes the corresponding bodies, thus completely establishing the point under discussion. The reduction of nitro-ketones gave the expected derivatives of p-amidophenol, *e. g.*, m-nitroacetophenone yielded amido-oxyacetophenone—



$NO_2C_6H_4COCH_3 =$ (NH_2, $CO.CH_3$, OH) .

Similar results were realized with m-nitrobenzophenone and with m-nitrophenyl-p-tolyl ketone.

Time forbids more than mere mention of Löb's reduction of the three nitrobenzoic acids and the theory he has proposed in explanation of their reduction; the hydrogenizing of pyridine with the production of piperidine; the obtainment of pipecoline from picoline; the probable reduction of all the higher homologues of pyridine; the conversion of quinoline into its tetrahydride and a trimolecular dihydroquinoline; that of quinaldine to its tetrahydride and a trimolecular dihydroquinaldine; Noyes and Dorrance's reduction of nitro-bodies containing substituents in the para-position, as well as the reduction of p-nitro-benzoic acid in acid solution by Noyes and Clements; Ahrens' efforts to reduce nitriles to primary amines; the conversion of eugenol into isoeugenol and the electrolytic oxidation of the latter to vanilline; Elbs' oxidation of p-nitrotoluene to p-nitrobenzyl alcohol; that of nitrosopiperidine to dipiperidyl; that of the toluene sulphonamides to benzoic sulphinides with the formation of saccharin, and finally the reduction of certain oxy-acids to dyes of great value.

My story is now told and it only remains for me to acknowledge my deep indebtedness to the various writers upon electrochemistry, whose thoughts and words I have freely drawn upon and utilized in preparing this very incomplete sketch of what must be regarded as merely the beginnings of the electrolysis of organic bodies. I feel, however, that you will grant that they have been most fruitful, and are indeed highly suggestive. It would be presumption on my part to suggest, for I am satisfied many new thoughts have come to you in listening to, as they have to me in preparing, this review, and to them will be added many more if we will but experiment in the field now opening to us.

I know of no more fitting conclusion to these imperfect and fragmentary paragraphs, than the words of Michael Faraday, truly a father of electrochemistry, who said:

"It is the great beauty of our science . . . that advancement in it, whether in degree great or small, instead of exhausting the subjects of research, opens the doors to future and more abundant knowledge, overflowing with beauty and utility to those who will be at the . . . . pains of undertaking its experimental investigation."

PAPERS READ.

[ABSTRACTS.]

PETROLEUM INCLUSIONS IN QUARTZ CRYSTALS. By CHAS. L. REESE, Johns Hopkins University, Baltimore, Md.

UNUSUAL evidence of cavities in quartz crystals containing petroleum. Crystals from Diamond P. O., Marshall County, Alabama.

AN ELECTRIC FURNACE FOR USE WITH THE 110-VOLT CIRCUIT. By N. MONROE HOPKINS, Washington, D. C.

VOLUMETRIC ANALYSIS. By G. E. BARTON, Millville, N. J.

ON THE FACILITIES FOR STANDARDIZING CHEMICAL APPARATUS AFFORDED BY FOREIGN GOVERNMENTS AND OUR OWN. By HENRY S. PRITCHETT. Read by L. A. FISCHER, U. S. Coast and Geodetic Survey, Washington, D. C.

SOME NEW FORMS OF APPARATUS. By J. M. PICKEL, University of Alabama, Tuscaloosa, Ala.

A NEW APPARATUS FOR DETERMINING THE RELATIVE VISCOSITY OF THICK OILS AND SOFTNESS OF PLASTIC MATTER. By A. W. DOW, Washington, D. C.

VISCOSIMETER. By P. H. CONRADSEN, St. Paul, Minn.

ANALYTICAL CHEMISTRY.

(In charge of P. DE P. RICKETTS, Columbia University, N. Y.)

THE INFLUENCE OF TEMPERATURE UPON THE SPECIFIC ROTARY POWER OF SUCROSE. By W. H. WILEY, U. S. Department of Agriculture, Washington, D. C.

THE DETERMINATION OF WATER AND COKE IN COAL. By W. A. NOYES and N. M. AUSTIN, Rose Polytechnic Institute, Terre Haute, Ind.

NOTES ON THE DETERMINATION OF WATER IN COAL. By W. E. HILLEBRAND, U. S. Geological Survey, Washington, D. C.

ANALYSIS OF MIXED ACIDS. By CHAS. E. MUNROE, Columbian University, Washington, D. C.

A SIMPLE COLOR REACTION FOR THE DETECTION OF METHYL ALCOHOL. By SAMUEL P. MULLIKEN and HEYWARD SCUDDER, Mass. Institute of Technology, Boston, Mass.

THIS test is based on the oxidation of methyl alcohol to formic aldehyde, and the conversion of the latter compound into a red coloring matter by the action of sulphuric acid and resorcin. It can be very quickly carried out, and gives satisfactory results even in rather dilute solutions, and in the presence of many organic compounds.

[This paper will be printed in the Amer. Chem. Journal.]

DETECTION OF THE NITRO GROUP IN ORGANIC COMPOUNDS. By SAMUEL P. MULLIKEN and E. R. BARKER, Mass. Institute of Technology, Boston, Mass.

THE tests heretofore proposed for the detection of the nitro group all lack that directness and certainty that are to be desired in a qualitative reaction, or are of limited applicability. It is found that reduction of the nitro to the hydroxylamin grouping is a reaction that easily lends itself to the detection of nitro compounds of the most diverse types. The formation of rosaniline from aniline-red oil, iron, hydrochloric acid and nitro compounds is another useful qualitative reaction, though it is less reliable than the preceding test.

[This paper will be printed in the Amer. Chem. Journal.]

ELECTROLYTIC DETERMINATION OF TIN IN TIN ORES. By E. D. CAMPBELL and E. C. CHAMPION, University of Michigan, Ann Arbor, Mich.

THE ore is decomposed by two fusions with sodium carbonate and sulphur and the solutions of sodium sulpho-stannate filtered off. The stannic sulphide produced by adding excess of hydrochloric acid is oxidized by means of sodium peroxide to stannic chloride from which the tin is deposited electrolytically after transposing into the double ammonium oxalate. The manipulations are simpler than in the methods now in use and the results are very accurate.

THE DETERMINATION OF UNDIGESTED FAT AND CASEIN IN INFANT FECES. By HERMAN POOLE.

NEW METHOD FOR THE DETERMINATION OF ZINC. By A. C. LANGMUIR, New York, N. Y.

NOTE ON DETERMINATION OF ARSENIC IN GLYCERINE. By A. C. LANGMUIR, New York, N. Y.

THE ESTIMATION OF IRON AND ALUMINUM IN NATURAL PHOSPHATES. By H. W. WILEY and F. P. VEITCH, U. S. Department of Agriculture, Washington, D. C.

THE TEACHING OF CHEMISTRY.

(Under the leadership of F. P. VENABLE, University of North Carolina.)

SOME DANGERS OF THE ABUSE OF FORMULAS. By F. P. VENABLE, University of North Carolina, Chapel Hill, N. C.

WHEN Thomson made his memorable visit to Dalton in Manchester, nearly one hundred years ago, and was shown the system of symbols by which Dalton hoped to make clear his ideas as to atoms and their combinations, he was enthusiastic in his expressions as to the future usefulness of such a system. And, although the system was clumsy, and inadequate to the task of properly representing the great mass of chemical facts, it contained the valuable idea of graphic representation, which was to be ingeniously developed by later masters of the science.

It was through Berzelius next that chemical symbols were made simpler and clearer, and so manifest was their usefulness that they speedily claimed the further additional advantage of universal acceptance. Local acceptance only, the use by chemists of one nationality, or by the followers of one master, would have proved a most serious bar to the advancement of the science. We can imagine the confusion which would result from many different systems at present, but happily such a picture exists in the imagination only. The science has one universal language of symbols which those of every tongue can read and understand. The advantages of such a system need not be dwelt upon. I purpose rather pointing out a few possible dangers and abuses.

The simple application of symbols in the time of Berzelius has become more complicated as the science has developed and the knowledge of both composition and constitution of chemical bodies has increased. Intended at first to represent elements and single compounds, the symbols have been developed into complex formulas and these have been united into algebraic equations in the effort to make them represent as much as possible of the knowledge so laboriously acquired by multiplied experiments. It has become in truth the short-hand system of chemistry.

While its great usefulness is not to be underestimated, the limitations of the system should be duly recognized. In the first place, it can only partially represent the mathematical relations of the science. Again, there is no mode of indicating in an equation the physical forces which always accompany chemical reactions. These relations are of especial importance and yet there is no adequate mode of expressing them. It is questionable how far such important relations as the electro chemical and those of chemical affinity itself can be deciphered from them. Certain it is that the best constructed equation frequently represents only one of many reactions occurring at the same time and under the same conditions and there is no way of indicating these conditions. Often an equation upon which much stress is laid, especially in organic chemistry, is really a secondary and subordinate one and, since this is not to be indicated by the formula, the student may be misled.

Such questions as whether all of these more or less desirable matters can ever be represented by any system, or whether the present system is not the most perfect which we can hope for, do not come within the field of the present discussion. It is sufficient to point out that the system is not flawless. We must all acknowledge that it is indispensable to chemists and, being something more than a mere short-hand system, greatly aids him in his work. For these reasons all young chemists must be properly initiated and inducted into the mysteries of the system so that they too may belong to the great order of the *illuminati*.

The great value of symbols and formulas in teaching chemistry is not to be questioned. Their use is essential in giving a quick grasp and a clear mental picture of what takes place; but abuse of them is quite possible and I will briefly point out some of the dangers which lie in this direction. It must be constantly borne in mind that these formulas do not

constitute the science of chemistry but are merely an abbreviated mode of stating some of the facts while many others do not admit of such brief, graphic representation. So much stress is sometimes laid upon these symbols and so much store set upon their manipulation, that the student gathers the impression that he knows and understands much of the science when he can glibly rattle off a few of them; and he devotes much time to memorizing certain of them, which time would far better be spent in an attempt at grasping the great science itself.

It has occurred to me that it might be profitable to emphasize the following dangers of abuse:

1. The danger of methodism. This is a danger common to much of the elementary teaching of the day. We have of late years many Schools of Methods established all over the country. These have their value in so far as they tell how knowledge may be imparted in an orderly, systematic, methodical manner but great care must be exercised lest the method should be magnified above the knowledge and the student go away with the empty method alone. I have known such schools where the whole subject was made ridiculous by extended dissertations upon the proper posture of the child in reciting, or in drinking water, or some equally subordinate matter, where the time at command was too valuable for more than brief mention of such details. And so we can all doubtless recall text-books on chemistry where large space is given to the arrangement, manipulation or completion of formulas and equations. Sometimes they are placed before the pupil like a dissected map or puzzle, shaken together or with some missing member to be supplied; certainly giving him one false idea, that such equations are to be worked out with pencil and paper where the effort should be to impress upon him the knowledge that such equations are only legitimate when the result of actual experiments and when proved in every particular by direct trial. Such juggling with formulas may induce a certain ingenuity, but may also be positively harmful so far as the acquiring of true knowledge is concerned.

2. The mathematical danger. The effort at placing chemistry upon a mathematical basis and so making of it a true science, according to the German definition, is a laudable one; but this end is not to be attained by a multiplication of problems based upon the time-honored rule of three and the simple algebraic equations made use of in chemical problems. Enough of this sort of mathematical work should be given to make clear the great underlying laws of chemistry — the indestructibility of matter, the constancy of combining proportions, etc. Beyond this, harm may be done to some minds not mathematically inclined. A bright, ingenious mind may revel in some of the abstruse and difficult problems which have been based on this rule of three while other minds may be frightened and confused. There is a temptation to a bright, mathematically-inclined teacher to add to these problems and exercises, but I maintain that in such case he is attempting to teach mathematics and not chemistry.

3. The mechanical danger. Many have seen this danger and given forth no uncertain note of warning against it. In graphically representing formulas, especially those of organic bodies, our mechanical limita-

tions are such that there can be no adequate picture given. Much must be left to the imagination. The effort is only to give an outline or a few points upon which to hang the mental picture. But young minds are so hopelessly matter-of-fact at times — and one of the most difficult of tasks is to transfer the vision of your imagination upon the mental retina of others. In the first place there is the blackboard with its plane surface and the hard chalk-lines. You are endeavoring to give a picture of some collection of wonderful symmetrical atoms bound together by strange invisible emanations of force and endowed with marvellous properties and powers. How can you hope to do it with such means? How can you devise mechanical means, — balls, strings, or rubber monstrosities which will properly aid you?

And yet these mechanical aids are helpful, for mere word painting is far too vague for the purpose. Only beware lest the idea be given that your rubber or glass toys or scratches on the board really look like the incomparable atoms themselves, and that chemical force displayed between them closely resembles a connecting wire, or a bit of glass tubing, or a white streak of chalk powder.

4. *The danger of idolatry.* To my mind, next to the achievement of language, the fully worked-out formula for a complex organic body represents the most wonderful accomplishment of the human mind. It is the result of years of toilsome experiment, of high theorizing, of ingenious logic. It is a building erected by some skilled artificer upon the delicate handiwork of other master workmen, all resting on the foundation stones of the science laid with infinite care and labor. It is a mighty epic to man's capacity for faithful toil, for self-sacrificing coöperation, for concentration of thought, for ingenuity of eye and hand and brain, and to his love for and deep yearning after the truth.

Yet, with all this granted as true, do not let us fall down before our formula and worship it. *Humanum est errare.* The worship of that which may desert us in the time of need is apt to lead to an unfruitful and unhappy skepticism. Be prepared to erect another more beautiful, because truer, building, if the first is overthrown. It is truth that we seek after and the building of our hands cannot be truth itself but at best only its fit temple.

I hope I shall not be misunderstood in what I have said about these dangers. I am not an iconoclast, but only plead for conservatism and moderation.

THE TEACHING OF ORGANIC CHEMISTRY. By IRA REMSEN, Johns Hopkins University, Baltimore, Md.

THE TEACHING OF INDUSTRIAL CHEMISTRY. By EDWARD HART, Lafayette College, Easton.

THE TEACHING OF ANALYTICAL CHEMISTRY. By WM. L. DUDLEY, Vanderbilt University, Nashville, Tenn.

INORGANIC CHEMISTRY.

(Under the leadership of H. L. WELLS, Yale University, New Haven, Conn.)

HISTORY OF DOUBLE SALTS. By HORACE L. WELLS, Yale University, New Haven, Conn.

THE recent work on double halogen salts was mentioned. Attention was called to the fact that the laws governing this class of compounds seem very intricate. For example, the chlorides do not always correspond to the bromides and iodides, and the double halides of closely related metals often differ. However, the law of simplicity of types seems very general, and it was pointed out that the types of double halogen salts with alkali metals and more negative metals are very similar, whether the latter be univalent, bivalent, trivalent, or quadrivalent. In this comparison, a type is understood as a definite ratio between the atoms of the two metals.

THE ALKALINE REACTION OF CERTAIN NATURAL SILICATES. By F. W. CLARKE, U. S. Geological Survey, Washington, D. C.

THE ACTION OF SOFT WATERS ON METALS. By ELLEN H. RICHARDS and WILLIS R. WHITNEY, Mass. Institute of Technology, Boston.

FERROUS IODIDE. By C. LORING JACKSON and I. H. DERBY.

Preparation, analyses and properties of FeI_2.
[To be published in the American Chemical Journal.]

THE ACTION OF CHROMIC ACID ON HYDROGEN. By CHAS. L. REESE, Johns Hopkins University, Baltimore, Md.

LUDWIG showed (Ann. Chem. (Liebig) 162, 47) that chromic acid oxidizes hydrogen in the presence of gypsum balls at ordinary temperatures, and from this experiment he made the general statement that hydrogen is readily oxidized by saturated solutions of chromic acid. The work here described confirms Ludwig's work, but shows that hydrogen is *not* oxidized by saturated solutions of pure chromic acid when agitated together in sealed tubes for a long time or even with the addition of from one to

ten per cent by volume of concentrated sulphuric acid. In presence of platinized asbestos oxidation however takes place.

At temperatures above 130° Centigrade, hydrogen is oxidized and at the same time saturated solutions of chromic acid give up oxygen. At temperatures below 130° the volumes of hydrogen oxidized and oxygen evolved were so small that the work will have to be continued to determine whether or not they were due to error of experiment. The work seems to point to the fact that hydrogen is not oxidized by chromic acid till a point of temperature is reached at which oxygen is given off from a solution of chromic acid in air.

[To be published in the American Chemical Journal.]

USE OF THE ELECTRIC CURRENT IN FORMING ALUMS. By J. L. HOWE, and E. A. O'NEAL, Washington and Lee University, Lexington, Va.

FORMATION of iron, aluminum and cobalt alums by the electrical current, with descriptions of several new alums (Rb and Cs), and unsuccessful attempts to form manganese and ruthenium alums in the same way.

[To be published in the Journal of the American Chemical Society.]

THE COLOR OF SULPHUR IN THE GASEOUS STATE. By J. L. HOWE and S. G. HAMNER, Washington and Lee University, Lexington, Va.

EXPERIMENTS to determine the real color of this vapor so variously given in textbooks.

A STUDY OF THE TELLURIDES. By CABELL WHITEHEAD, Treasury Department, Washington, D. C.

MAGNETIC FERRIC OXIDE. By W. L. DUDLEY, Vanderbilt University, Nashville, Tenn.

THE ACTION OF VARIOUS BASES ON METALLIC ARSENITES. By C. WELLINGTON, Amherst, Mass.

A NEW AND DELICATE TEST FOR NICKEL. By O. C. JOHNSON, Ann Arbor, Michigan.

THE solution of cobalt and nickel is treated on the water bath with sodium carbonate and bromine.

After thorough washing the resulting cobaltic and nickelic hydroxides, are treated with hot potassium iodide.

The liberation of iodine indicates nickel.

This test is exceedingly delicate and characteristic. In fact none of the ordinary metals interfere except lead, the peroxide of which also liberates iodine.

A NEW METHOD OF STANDARDIZING HYDROCHLORIC ACID. By F. K. CAMERON and J. A. EMERY, Cornell University, Ithaca, N. Y.

ORGANIC CHEMISTRY.

(Under the leadership of IRA REMSEN, Johns Hopkins University, Baltimore, Md.)

ON THE SULPHUR COMPOUNDS AND UNSATURATED COMPOUNDS IN CANADIAN PETROLEUM. By C. F. MABERY and W. O. QUAYLE, Case Scientific School, Cleveland, Ohio.

WE have separated from Canada petroleum a series of unsaturated hydrocarbons, C_7, C_8, C_9, C_{10}, and a series of sulphur compounds of the general composition $C_n H_{2n}S$, C_9, C_{10}, C_{11}, C_{12}, C_{13}, C_{14}, etc., and determined their composition by analysis; from some of these bodies sulphoxides have been formed, $C_n H_{2n} SO_2$

ON THE COMPOSITION OF COMMERCIAL PARAFFINE AND THE HIGHER CONSTITUENTS OF PENNSYLVANIA PETROLEUM. By C. F. MABERY, Case Scientific School, Cleveland, O.

THESE constitutents have been separated by distillation *in vacuo*, analyzed and in part the molecular weights determined, of the series CH_{2n+2}. The following members have been separated: $C_{14} H_{30}$, C_{15}, C_{16}, C_{17}, C_{18}, C_{19}, C_{20}. Of the higher members a complete series has been separated and analyzed, but the difficulties in the way of determining the molecular weights have not been entirely overcome.

ON THE CONSTITUENTS OF CALIFORNIA PETROLEUM. By C. F. MABERY and E. J. HUDSON, Case Scientific School, Cleveland, O.

IN continuing our study of these petroleums from different sources, we find a great variation in composition. Some contain a very large proportion of aromatic hydrocarbons, as benzol, toluol, xylols, etc., and with smaller amounts of naphthenes; while others contain less aromatic hydrocarbons, but consist largely of naphthenes.

SOME EXPERIMENTS ON THE ADDITION OF HYDROGEN TO ACETYLENE. By C. F. MABERY and H. L. SCHROM, Case Scientific School, Cleveland, O.

ALL our results were negative, and we tried sodium amalgam, metallic aluminum, zinc and sulphuric acid, electrolysis, passing acetylene into the decomposing cell.

We also passed acetylene over platinum sponge, mixed with hydrogen. We also tried the action of zinc and sulphuric acid on acetylene copper, with unsatisfactory results.

We find that ethylene is absorbed by sulphuric acid even at 160°-170° with extreme slowness.

CERTAIN PECULIAR REACTIONS OF THE TRIBROMNITROBENZOLS. By C. LORING JACKSON, Harvard University, Cambridge, Mass.

ACCOUNT of the action of sodium malonic ester, sodium aceto-acetic ester, and sodium ethylate on tribromdinitrobenzol and related substances with a view to determining why the Br. is replaced by H. Also a study of the removal of nitro groups from these compounds.

[To be published in the American Chemical Journal.]

THE PROPYL PHOSPHINES. By PETER FIREMAN and E. G. PORTNER, Columbian University, Washington, D. C.

THE ACTION OF ETHERS ON PHOSPHONIUM IODIDE. By PETER FIREMAN and ERNESTINE FIRKMAN, Columbian University, Washington, D. C.

CAMPHORIC ACID: SYNTHESIS OF THE NEIGHBORING XYLIC ACID. By W. A. NOYES, Rose Polytechnic Institute, Terre Haute, Ind.

ON TETRAPHENYLMETHANE. By M. GOMBERG, University of Michigan, Ann Arbor, Mich.

HYDRAZO- AND AZO-DERIVATIVES OF TETRAPHENYLMETHANE. By M. GOMBERG and A. C. CAMPBELL, University of Michigan, Ann Arbor, Mich.

OXYINDULINE — A NEW BLUE DYESTUFF. By SAMUEL P. MULLIKEN and W. KELLEY, Mass. Institute of Technology, Boston, Mass.

THIS compound, $C_{36}H_{26}N_4O_4$, is readily formed by the action of heat on para-amidophenol hydrochlorate. It closely resembles the indulines in its reactions, except in showing phenolic properties. Sulphonated, it is a fast and direct blue wool dye.

[This paper will be printed in the American Chemical Journal.]

THE BENZALDOXIMES. By F. K. CAMERON, Cornell University, Ithaca, N. Y.

CERTAIN ALKALOIDAL PERIODIDES AND THE VOLUMETRIC ESTIMATION OF ALKALOIDS AS HIGHER PERIODIDES. By A. B. PRESCOTT and H. M. GORDIN.

IN continuation of previous work upon perhalides of organic bases (Prescott, Gomberg, Trowbridge and others, 1895-98: J. Am. Chem. Soc., *17*, 775, 859; *18*, 28, 91, 331, 347; *19*, 322, 558), the authors have now obtained the following, as compounds of quite constant composition:

Atropine hydriodide octaiodide, $C_{17}H_{23}NO_3 . HI . I_8$
Strychnine hydriodide hexaiodide, $C_{21}H_{22}N_2O_2 . HI . I_6$
Brucine hydriodide hexaiodide, $C_{23}H_{26}N_2O_4 . HI . I_6$
Morphine hydriodide triiodide, $C_{17}H_{19}NO_3 . HI . I_3$
Aconitine hydriodide hexaiodide, $C_{33}H_{45}NO_{12} . HI . I_6$

The morphine periodide obtained constant in the given conditions is the same found by Jörgensen in 1870. The composition of the higher periodide of aconitine above written, on the alkaloid formula of Dunstan, is given provisionally. The same conditions insure the formation of the periodide of caffeine obtained by Gomberg in 1896, namely, caffeine hydriodide tetraiodide.

The conditions necessary for these formations are only those of precipitation from quite dilute water solutions with a constant excess of iodine from first to last, the alkaloid being in a salt or acid solution. In such conditions these are the highest periodides that can be formed. In precipitation from water solutions with excess of the alkaloidal salt, lower periodides are formed in case of atropine, strychnine, brucine, aconitine, not in case of morphine. Some of these lower periodides the authors have analyzed, and others will be examined. In character and behavior the periodides of the vegetable alkaloids agree in general with the perhalides of pyridine and its more immediate derivatives, as these have been previously studied in the same laboratory. Also the reactions of these periodides of the bases of the pyridine type are generally the same as those of caffeine pentiodide, formed when caffeine is in acidulated solution, as found by Gomberg. And these bodies are to be studied with perhalides in general, and with the question of what orders of bases are capable of forming perhalides, and the relation of these, in structure, to the double halides. The production of a periodide of bromtriphenylmethane, about to be published by Gomberg, is of interest as an organic perhalide destitute of any element of the nitrogen family, as was also the iodonium perhalide obtained by Victor Mayer in 1894, and the sulphon periodide reported in this country by Kastle in the same year.

The higher periodides of the vegetable alkaloids, as named above, are sufficiently stable to give very constant results in analysis, both for the additive iodine and for that of the hydriodide. Obtained in crystalline forms they were found to have the same composition as the amorphous precipitates after washing and drying. In the volumetric procedure, however, washing is avoided, an aliquot portion of the filtrate being taken, in which to titrate back with the thiosulphate for the excess of iodine added. In this way is obtained a very accurate measure of the iodine consumed in supplying the additive iodine of the periodide. The more firmly bound iodine of the alkaloidal hydriodide, not responding to reducing agents, is supplied by the iodine of the potassium iodide, that of Wagner's reagent, or iodo-potassium iodide, always employed. This probable explanation of the reaction of the precipitation was verified by the authors in a quantitative investigation at some length.

The results of analysis of the periodides were found to be well sustained by constant results in volumetric estimation, the alkaloid to iodine factors calculated from the chemical formulæ of the periodides being used in every case for the volumetric work, with agreement quite satisfactory, as presented below. The calculation for each alkaloid was made upon its chemical formula, as given in a previous paragraph:

RATIO OF ALKALOID TO 1 OF IODINE.	TOTAL IODINE CALC.	TOTAL IODINE FOUND.	ADDITIVE IODINE CALC.	ADDITIVE IODINE FOUND.	PER CENT SOL. MADE.	PER CENT FOUND BY VOL. ANAL.
Atropine,	79.74	79.48	70.88	70.98	0.50	0.47
0.2849		80.13		71.15	0.40	0.36
		79.62			0.30	0.28
Strychnine,	72.66	72.70	62.28	62.14	1.000	1.026
0.4390		72.54		62.34	0.612	0.633
Brucine,	69.21	69.04	59.32	59.19	1.000	0.999
0.5179		69.04		59.37	0.5	0.497
Morphine,	64.03	63.80	48.02	48.29	0.516	0.525
0.74918		63.54		48.44	0.259	0.257
					0.100	0.105
Aconitine	57.85	55.93	49.58	49.03		

In the case of atropine the authors obtained two mercuric iodides, respectively $C_{17}H_{23}NO_3HI.HgI_2$, and $(C_{17}H_{23}NO_3HI)_2HgI_2$ They can be prepared by treating the periodide with metallic mercury, and in other ways described.

In the volumetric estimations, the iodine solution is made of decinormal strength, with sufficient potassium iodide, and a corresponding strength of solution of sodium thiosulphate is used. To a known volume, constituting an excess of the iodine solution, a measured quantity of an aqueous salt solution of the alkaloid is added gradually, the mixture is shaken until the liquid becomes clear, when an aliquot part of all is filtered off and the excess of iodine determined by titrating with the thiosulphate to an end reaction with starch.

The authors have elaborated a volumetric method of opium assay, upon the following plan: The opium alkaloids are set free by action of ammonia with certain solvents applied to the powdered opium. The free narcotine, paraverine, codeine and thebaine are then removed by percolation with benzene (benzol), after which the morphine is wholly taken out by percolation either with amyl alcohol or with acetone. Both these solvents have been used. The percolation is effected well by admixture of dry common salt with the opium powder. The solvent is evaporated from the percolate of morphine, and the residue taken up with lime water, whereby the alkaloid is purified from color and other extraneous matters. The

filtered lime solution is acidulated, and the morphine in it estimated directly as periodide. One gramme of opium is quite sufficient, and two or more final titrations can be obtained from this quantity.

In the results of opium assay by this method, repeated operations give figures agreeing quite closely with each other. So far as compared with the pharmacopœial assay, the results do not give lower percentages, but in some cases give higher percentages.

Other drug assay methods, with volumetric estimation of the alkaloid as periodide, are in progress of investigation.

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ON TRUE AND BIS-NITROSO COMPOUNDS. By EDWARD KREMERS, Madison, Wis.

VON BAEYER'S explanation of the differences of the menthene nitrosochloride prepared by Sieker and Kremers and that prepared in his laboratory, viz., that the former consists of a simple molecule and the latter of a double molecule is not supported by experiment. All white nitroso-addition products of unsaturated hydrocarbons examined thus far reveal a double molecule in solution, which double molecule has a greater or lesser tendency (according to stability as to temperature or solvent employed) to dissociate into the blue simple molecule. In certain cases complications are brought about by the splitting off of acid molecules from the nitrosochlorides or nitrosates. The peculiarity of ether, when used as a solvent, to cause the aggregation of a number of simple molecules to one very large molecule is another illustration of an exception to the general rule (as far as observations go) mentioned above.

Thus far the action of alkyl nitrites and acids on unsaturated compounds has been studied primarily from the standpoint of addition, viz , of $\left|\begin{matrix}NO\\Cl\end{matrix}\right.$, $\left|\begin{matrix}NO\\ONO\end{matrix}\right.$ and $\left|\begin{matrix}NO\\ONO_2\end{matrix}\right.$. This addition evidently is but one phase of the reaction now generally employed in the preparation of these compounds. Dinitrophellandrene, nitroso-pulegone, etc., may be mentioned as exceptions.

PHYSICAL CHEMISTRY.

(Under the leadership of T. W. RICHARDS, Harvard University, Cambridge, Mass.)

PROGRESS IN PHYSICAL CHEMISTRY. By THEODORE W. RICHARDS, in charge of Physical Chemistry, Harvard University, Cambridge, Mass.

IN these days of far-reaching specialization, the would-be speaker upon any subject is between a new Scylla and a new Charybdis. In order that his production should be comprehensible to those outside of the specialty

it must almost inevitably be made boresome to those within the fold; but, on the other hand, that which is new to the specialist in his own topic is apt to be quite too new to the layman. Either popularizing or specializing is likely to wreck the speaker's purpose by inducing at least a part of his audience to slumber, and this danger is especially imminent after dinner on a hot day which has been filled with mental effort. In this brief address, which Professor Smith has entrusted to my care, I shall probably run foul of both obstructions; but this irregular course will have the great concomitant advantage of permitting each class of hearers to obtain a few minutes of much needed repose.

We are rather accustomed to look upon physical chemistry as being a very modern invention, and in one sense we are not wrong in so doing. But after all, many of the fundamental generalizations of physical chemistry are by no means recent, and some of them are really old. Leaving out of account the probable discoveries in the subject made by Adam and Eve in the garden of Eden, it is certain that the philosophers and alchemists were as much interested in those phenomena which lie on the borderline between physics and chemistry as in those which were purely physical or chemical. Indeed, the sharp line between the two subjects, drawn with so much emphasis twenty or thirty years ago, did not then exist. It is interesting to note that this sharp line is now rapidly being erased; we are realizing more and more that the laws which govern one class of phenomena are applicable also to the other. Thus the cosmic working of the mind of man swings back and forth; will it ever come to rest upon the absolute truth?

In the middle of the seventeenth century, while this country was in an infantile condition, and Harvard College was little more than embryonic, Robert Boyle discovered his law relating to the contraction of gases under pressure,—one of our most fundamental conceptions to-day. Lavoisier, before his tragic death in the Reign of Terror, forced upon a somewhat reluctant world the idea of the conservation of mass,—an idea which perhaps had been half assumed by some before — and in so doing laid a corner-stone of the great structure which was to follow. Only a few years later, Dalton, Avogadro, Ampère, Gay-Lussac, Dulong and Petit, Davy and Faraday that prince of pioneers, with others less famous, made great additions to the world's thought in a physico-chemical direction. As far back as our forties Julius Robert Mayer and Helmholtz had acquired a clear grasp of the conservation of energy, while the other great law of energy had been partially realized by Sadi Carnot two decades before. Hittorf's classical research on electrolytic conductivity, with Wilhelmy's epoch-making study of the speed of a reaction, a research upon the lines laid down by Wenzel and Berthollet so much earlier, took place over forty years ago. Only a decade later Guldberg and Waage laid down in unequivocal and comprehensive terms the fundamental law of mass-action, which is the basis of Wenzel, Berthollet, and Wilhelmy's observations, as well as of the progress and equilibrium of every other chemical change.

Why, then, with these foundations laid so far in the past, are we inclined to call physical chemistry a brand-new structure? We all know that a part at least of the retarded development was due to the difficulty of dealing with solutions, which seemed anomalous in so many ways. Van't Hoff, by showing that a substance in solution followed many of the laws which would govern it in the aëriform state, and Arrhenius, by explaining in a simple way the differences between solutions conducting electricity and those which are non-conductors, cleared the track of these obstructions; hence, for the last ten years the pace has been rapid. But it seems to me that there is another reason for the tardiness of the recognition of the importance of physical chemistry to be found in an unfortunate tendency observable sometimes in both chemists and physicists, a tendency which I am afraid we must call prejudice. Not only have untenable theories been held long after their time, but whole fields of study have been neglected by most chemists and physicists, because they lie on the borderline between the two sciences.

The average physicist only half realized that one of the most important relations of his great new force electricity is chemical, while the chemist does not always realize even to this day that Wheatstone's bridge and the telephone are chemical tools just as legitimate as, and no more "physical" than the thermometer, or the time-honored balance, which extricated his predecessors from so hopeless a slough a hundred years ago. The day is fast approaching, however, when both chemist and physicist will welcome every mode of acquiring more light upon the absorbing topic which engages them in common — the study of the ultimate laws and structure of the universe.

It is a pleasure to think that one of the foremost of the brilliant men who have joined in advancing this wished-for end — this union of the resources of physics and chemistry to a common purpose — since the last meeting of the American Association has had his services recognized and his opportunity enlarged by his own university of Leipzig. Wilhelm Ostwald's new laboratory for physico-chemical research is the second important university building for this purpose which has been erected in Germany, the first having been built at Göttingen for the brilliant Walther Nernst; and the fact that Ostwald should at last have obtained a material outfit worthy of his unusual mental equipment is welcomed with enthusiasm by his many warm friends. We all know what a profound effect Ostwald's surprising book, his timely Zeitschrift, and in general his broad and progressive point of view, have had upon the development of both chemistry and physics, and it has been a matter of some surprise to many that so great an influence should have emanated from a laboratory so insignificant as the old Zweitem Laboratorium, so-called. The new building, although not very large, is in many respects a model; the architect and director of any kind of scientific workshop could not fail to obtain valuable hints from the detailed statement of it contained in the recently published appendix to the Zeitschrift für Physikalische Chemie. What a pity that America should allow Germany to outstrip us so far in devo-

tion to the ideals of pure science! How long will it be before we build laboratories especially for physical chemistry, or even in many colleges allow a considerable share of old buildings to this end? And yet this physical chemistry now comprehends all of the field of theoretical chemistry, except a certain kind of reasoning concerned with the structure of organic substances, and the purely chemical part of the mysterious classification called the periodic system of the elements.

Very few of the processes of nature are simple in their proximate causes or their outward manifestations, however simple the grand underlying principles may be in their ultimate essence. The old maxim, by which theories were so often consciously or unconsciously judged, "Our theory is so simple that it must be true," is a dangerous guide. Geber's old notion that the whole world consists of sulphur and mercury, and the topsy-turvy delusion of phlogiston relied largely on this maxim for support, and it behooves us to avoid similar mistakes. When the ancient idea of luck had been eliminated from scientific reasonings, mankind admitted that every phenomenon is a function of its controlling causes; but that all the mathematical relations should be capable of solution, although to be sure only with the aid of the potent modern methods, is a new conception. In the old days, problems in chemistry which could not be solved by simple arithmetic or at best by elementary algebra, were considered incapable of quantitative solution; now, the higher mathematics is a facile tool in the hand of many an eager chemist. Even that mystery of mysteries, the smallness of the yield in the preparation of organic substances, has a flood of light shed upon it by the phase-rule and the mass-law!

No one who is familiar with the facts can doubt that the mathematical point of view will prove in the future more and more useful to chemists, as well as to the new physical botanists and zoölogists, who are bringing it to bear on their transcendently recondite problems. These last-named investigators will follow in the footsteps of chemists, as chemists have followed in the footsteps of physicists.

The advance of the mathematical point of view in chemistry has brought with it an entirely new danger, as well as a new field of power. The accuracy of a result of course depends upon the accuracy of the foundation upon which the reasoning rests; and accurate mathematical processes may lead to wholly erroneous conclusions if they are based upon incomplete data. In chemistry this cause of error is especially prominent, because of the great complexity of most of the phenomena and the fact that they are often modified by subordinate influences. For this reason a physicist, used to simple phenomena and less complex effects, is especially apt when he deals with problems allied to chemistry, to erect a large superstructure of mathematical reasoning possessing the semblance of reality upon a paper foundation, and be drearily awakened some fine day by the collapse of his air castle. The only mode of guarding against this subtle cause of disaster is to bring as much skill into every step of the experimentation as into the pure reasoning based upon the supposed facts. Here the physicist is seriously hampered by his lack of

knowledge of chemistry, as well as by his usual repugnance to dealing with glass vessels and liquids; while, on the other hand, the chemist is equally hampered by his inbred dislike of brass instruments, and his imperfect acquaintance with the manipulation of his new sensitive tools.

In short, one must be an accomplished chemist, physicist, and mathematician in order to attain the highest results in modern theoretical chemistry, and the number of men who have the time or ability to acquire this threefold education can never be large. All honor to Van't Hoff, Ostwald, Nernst, and the others who come nearest the high ideal! While it is true, however, that few men can hope to attain the highest, it does not follow that the rest of us cannot be of great use. Each man can be of value in his own particular sphere; it is only necessary that he should work faithfully with a single eye to the truth, that he should be as free as possible from prejudice, and that his published work should be as accurate as he can make it. A well conducted organic synthesis, a few carefully determined solubilities, will in the end be more valuable to the progress of science than a false generalization, no matter how ingenious the latter may be. But how great is the responsibility of the collector of facts! for if his observations are false, his work is of less value even than that of the false theorist; it has not even ingenuity in its favor, and is worse than useless. Ostwald has more than once pointed to the responsibility attending publication, and we should all do well to heed his warning.

A comprehensive design which I had once harbored of giving you a résumé of the year's work in physico-chemical research throughout the world has been relinquished because of the great number of small papers which could not be treated satisfactorily in the brief space of an evening's talk. The subject of stoichiometry, in Ostwald's rather comprehensive interpretation of this word, has received this year the attention usually accorded to it. Solutions have still occupied many able men, without having by any means had their possibilities exhausted. Van't Hoff's admirable little book upon double salts has already begun to exercise an effect upon the chemical world, which the scattered and less illuminating papers of his students could not have been expected to exercise. Dr. Gibbs' interesting address upon this important topic will undoubtedly excite further study on the whole question of the so-called "molecular compounds," which are so little understood and so hard to reconcile with our only partially satisfactory ideas of quantivalence. The far-reaching subject of chemical equilibrium is receiving more and more attention every year. There has been great activity in the fascinating field of electrochemistry, and it is pleasing to see that some of the fundamental notions of this new science are coming to be recognized by the analyst and the technical chemist. Not least among the startling events of the year has been the supposed discovery of a number of new elements, crypton, neon, matargon, coronium and etherion; if these really exist, we have here a series of brilliant chemical discoveries made solely by means of physical instruments and operations.

The most important of these interesting investigations are undoubtedly as well known to you as to your speaker, for in this day the sources of information are equally open to all; hence it would be a work of supererogation for me to discourse upon them in detail, even if there were time to do so. I prefer therefore to call your attention to some unpublished work with which you can hardly be so familiar; I mean the physico-chemical problems which have enlivened the last winter's laboratory work at Harvard. Since these covered a somewhat extensive field, their exposition may serve the double purpose of illustration and information. It is a pleasure to state that most of these researches would not have been thought of without the inspiring example and precept of the great men of whom I have spoken. The host of interesting investigations thus prompted in all civilized lands afford the best possible proof of the value of the modern physico-chemical hypotheses.

Dr. Gordon, the Harvard assistant in physical chemistry, has finished a very interesting series of measurements of the potentials of galvanic cells composed of metallic plates immersed in fused salts at high temperatures. After overcoming experimental difficulties, too numerous to mention, he succeeded in obtaining constant values which agree remarkably with Nernst's formula and throw interesting light on the degree of dissociation existing in fused salts. Mr. Edward Collins has nearly finished an elaborate attempt to verify Faraday's law with rigid exactness; an attempt which has met with greater success than any previous one.

Mr. G. N. Lewis made a series of careful measurements of the change of the potential of numerous reversible electrodes with the temperature, as well as a comprehensive revision of Meyer's inaccurate work on concentration cells involving amalgams of different strengths. In the thermodynamic discussion of the results, Mr. Lewis arrived at some very interesting conclusions concerning strong as well as weak solutions of metal in mercury and extended his experiment and mathematical analysis to the consideration of the potential of the unamalgamated metal in a solution of one of its salts. It is needless to say that this question is one of wide significance, but lack of time prevents my doing more than call attention to it now. Mr. Lewis's preliminary paper will appear early in the fall.

Mr. F. R. Fraprie spent much time in studying the eccentricities of inversion temperatures and transition intervals exhibited by the double sulphates of potassium and manganese. This problem proved to be far more complex and interesting than the similar case involving magnesium instead of manganese, a case which has been so carefully investigated under Van't Hoff's supervision. While Mr. Fraprie was not able to push the matter to completion, he obtained data enough to enable one to plot many of the most essential curves, and to draw a mental sketch of the situation. It is hoped that this work may be continued during the present year.

Mr. Faber, in the course of a research having a more practical end as its chief aim, made a series of determinations of the solubilities of argentic halides in solutions of sodic thiosulphate, and obtained data which may be of use in determining the mechanism of this reaction. Messrs. Har-

rington and Henderson made a few interesting observations on some cases in which the dissolving of a solid in a solution caused a lowering instead of a rise in the boiling point, and Mr. Churchill made a careful determination of the melting point of crystallized Glauber's salt. The aim of this last labor was to secure a new fixed point for the standardizing of thermometers; and we succeeded in showing that the point was as easily obtained and as constant at 32.484° as could be desired. This means of verifying thermometers will be a great boon to those who have not a standard instrument at hand. The paper will appear in the September numbers of the Zeitschrift für physikalische Chemie and the American Journal of Science.

Besides these varied researches, a protracted study of the causes of the occlusion and the unequal release of gases by the oxides of metals formed from the nitrates, occupied most of my spare time. It became evident that the excess of oxygen usually present in such material has a tendency to work its way out by a process of dissociation and recombination which reminds one of the old-fashioned explanation of electrolysis. The nitrogen, not being able to escape in this fashion, is retained. This paper has just appeared in the Proceedings of the American Academy.

In addition to these, several other researches were also in progress, which, although they belong strictly to the domain of inorganic chemistry, would never have been undertaken but for the theoretical interests involved. Prominent among these were the revisions of the atomic weights of cobalt, nickel, uranium, strontium and calcium, undertaken by Dr. Cushman, Messrs. Baxter and Merigold, and myself.

Of course it sometimes happens that physico-chemical problems involving the use of complicated apparatus may be more readily solved in a physical than in a chemical laboratory, and in any case the coöperation of these two departments is highly advisable. I am happy to say that Dr. Duane and others have been conducting such chemico-physical investigations at the Jefferson Physical Laboratory of Harvard College, but of these I have no authority to speak.

When one has been discussing the past and present progress of any subject it is natural that the mind should turn to the future also. What fields are likely to prove the most fruitful in the years to come? What researches will probably best advance the interests of science, and therefore of the life of man? Prophecy is always an uncertain business; but if one recognizes the uncertainty, it has few dangers and becomes at least amusing. In this case, however, it presents few difficulties.

The whole field of physical chemistry is so fruitful when treated by modern methods, that one can hardly single out any one section as especially unpromising. Almost every subject is worthy of research; a more important question is the *spirit* in which the research is to be undertaken.

There have always been two parties as regards any question brought forward by mankind for discussion: the conservative party and the radical party. The former has a tendency to cling to old ideas simply because they are old, and the latter has a tendency to adopt new ideas simply be-

cause they are new. It seems to me that neither of these tendencies is legitimate. One should seek new points of view continually, but he should hold to that which is good until something is proposed which seems to him better. In every case he should weigh the respective arguments for and against the new point of view with a mind as free as possible from prejudice, and with a single eye to the truth. In short, the ideal investigator is the scientific independent, the chemical "mugwump." Is it too unreasonable to hope that the problems of the twentieth century will be dealt with in this thoughtful but untrammeled fashion?

We Americans rejoice in having on our side of the ocean the world-renowned names of several great men, of Wolcott and Willard Gibbs, of James Crafts, Edward Morley, the late Josiah Parsons Cooke, and others, who have combined chemistry with physics and mathematics; but nevertheless, one must admit that America has not done as much as one could wish toward building up the fabric of modern physical chemistry. Although science is world-wide, and scientific men should be cosmospolitan, the existence of this Association proves that there is a a patriotic side to the matter too. While welcoming the truth, wherever it is discovered, let us then do all we can to further its emanation from American laboratories and writing desks.

SOME PROPERTIES OF LIQUID AMMONIA. By E. C. FRANKLIN and C. A. KRAUS, University of Kansas, Lawrence, Kan.

1. Liquid ammonia as a solvent.
2. Metathetic reactions between certain substances dissolved in liquid ammonia.
3. Ebulliscopic determination of the constant of molecular elevation of liquid ammonia.
4. Determination of the heat of volatilization of liquid ammonia at boiling point at atmospheric pressure.
5. Electrolytic conductivity of certain substances dissolved in liquid ammonia.
6. The temperature coëfficient of conductivity of liquid ammonia solutions of certain salts.

Liquid ammonia as a solvent. We have studied the behavior of liquid ammonia as a solvent toward some 500 substances. These experiments have shown ammonia to possess remarkable powers as a solvent for salts, while of the 350 organic compounds tested, over 250 were found to be readily soluble in ammonia.

Metathetic reactions between certain substances dissolved in liquid ammonia. We have studied qualitatively the metathetic reactions, in ammonia solutions, between the nitrates of lithium, sodium, potassium, calcium, strontium, barium, magnesium, zinc, cadmium, manganese, cobalt, nickel, lead, mercury, silver, and copper on the one hand and the chloride, bromide, iodide, chromate, borate, and sulphide of ammonium

on the other. As the fact that ammonia is a remarkably good electrolytic solvent would indicate, metathetic reactions take place rapidly and completely in ammonia solutions.

Ebulliscopic determination of the constant of molecular elevation of liquid ammonia. The molecular elevation of the boiling point of liquid ammonia has been determined for twenty substances. The boiling vessel consisted of a vacuum-jacketed tube in which the boiling was accomplished by passing a current of electricity through a coil of platinum wire sealed into the vessel. The boiling point elevations were measured by means of a Beckmann thermometer. No attempt was made to condense the ammonia and return it to the boiling tube; the amount of solvent was therefore determined after each reading by weighing the whole piece of apparatus. The molecular elevation constant was found to be 3.4 for water, ethyl alcohol, and a number of other substances. Several substances showed considerable variation of the constant with the concentration, while benzene gave value of about 2.5.

Determination of the heat of volatilization of liquid ammonia at its boiling point at atmospheric pressure. We have at the present time the method so well in hand that we feel confident of being able to make a very satisfactory determination of the heat of volatilization of ammonia at the boiling point. The method consists in boiling the liquid — by means of the electric current, and arriving at the amount of energy consumed in converting a known weight of the liquid into gas, by measuring the amount of electricity passed and the fall of potential within the boiling cell. Preliminary experiments have given values between 326 and 332. The heat of volatilization calculated from a number of formula varies between 323 and 366 cal.

The electrolytic conductivity of certain substances dissolved in liquid ammonia. — We have added a number of conductivity measurements to the list published by Cady. A considerable number of organic compounds are conductors in ammonia solutions.

The temperature coëfficient of liquid ammonia solutions of certain salts.— Measurements of the temperature coëfficient of several substances in ammonia solution gave a rate of increase of conductivity of about 2 per cent per degree between — 70° and the boiling point of liquid ammonia. Between — 10° and 35° the solutions pass a maximum of conductivity. Above the maximum the conductivity diminishes regularly as the liquid approaches its critical temperature, at which point, the solution becomes practically a non-conductor.

In a special part we draw attention to what seems to be true, that, of all known substances, ammonia most closely resembles water in all those properties which give to water its unique position among solvents. In its capacity as a general solvent for salts it is secondary to water, but superior to all other solvents. It closely approaches water in its power of dissociating electrolytes; some salts conduct even better in ammonia solution than they do in aqueous solution. It unites with many salts forming a group of bodies analogous to the salts with water of crystallization. Its

heat of volatilization is greater than that of any other liquid with the one exception of water. Its specific heat is quite as great as that of water, while its molecular elevation constant is lower than that of any solvent for which measurements have yet been made. The dielectric constant of ammonia has not been measured, but it should, if Nernst's suggestion is to hold good, approach that of water.

[The paper will be published in the American Chemical Journal.]

THE SOLUBILITY OF DI-IONIC SALTS OF WEAK ACIDS IN SOLUTIONS OF STRONGER DI-IONIC ACIDS. By ARTHUR A. NOYES and DAVID SCHWARZ, Massachusetts Institute of Technology, Boston, Mass.

A MATHEMATICAL expression of the theory of the phenomenon in question is derived from the law of mass action and the hypothesis of electrolytic dissociation. Experiments are communicated on the solubility of silver benzoate in water and in solutions of nitric acid and of chloracetic acid of various concentrations. The actual values of the solubility are shown to be in close accordance with the predictions of the theory.

THE SOLUBILITY OF DI-IONIC ACIDS IN SOLUTIONS OF THE DI-IONIC SALTS OF OTHER ACIDS. By ARTHUR A. NOYES and EDWARD S. CHAPIN, Massachusetts Institute of Technology, Boston, Mass.

THIS paper discusses another phase of the theory considered in the preceding communication. Experiments on the solubility of benzoic acid in water and in solutions of sodium acetate and of sodium formate are described. The results confirm satisfactorily the theoretical principles involved.

THE SOLUBILITY OF TRI-IONIC BASES WITH SOLUTIONS OF DI-IONIC SALTS OF WEAK BASES. By A. A. NOYES and E. S. CHAPIN, Mass. Institute of Technology, Boston, Mass.

THE SOLUBILITY OF IODINE IN DILUTE POTASSIUM IODIDE SOLUTIONS. By A. A. NOYES and L. J. SEIDENSTICKER, Mass. Institute of Technology, Boston, Mass.

THE RATE OF REACTION BETWEEN SILVER ACETATE AND SODIUM FORMATE: A REACTION OF THE THIRD ORDER. By A. A. NOYES and GEORGE T. CATTLE, Mass. Institute of Technology, Boston, Mass.

ON THE INFLUENCE OF SILICON ON THE HEAT OF SOLUTION OF COKED CAST IRON. By E. D. CAMPBELL and W. E. HARTMAN, University of Michigan, Ann Arbor, Mich.

THE paper details the calorimetric results obtained by dissolving in a slightly acidified solution of ammonium cupric chloride, a series of thirteen irons ranging in grade from mottled to ferro-silicon, and in silicon content from .898 to 13.626. From the data obtained the following conclusions are drawn: First, when either on account of the temperature in the blast furnace at the time the iron was made or on account of sufficiently high silicon, approximately 1.4%, there is very little combined carbon in the iron, the heat evolved by the oxidation on solution of the silicon is proportional to the amount of silicon in the sample. Second, the above does not hold true when the amount of combined carbon is very high owing to the amount of heat absorbed in the decomposition of the carbon compounds or those of carbon with iron and silicon. Third, That the compound of iron and silicon has a heat of formation of not less than 14700 calories for each gramme molecule. Fourth, that when the sample contains sufficient or nearly sufficient silicon to correspond with the empirical formula $SiFe_3$ the substance is unacted on by slightly acid ammonium cupric chloride.

ON THE PASSAGE OF BUBBLES THROUGH MEDIA OF DIFFERENT DENSITIES. By C. GILBERT WHEELER, Chicago, Ill.

SOME BOILING-POINT CURVES. By F. K. CAMERON and E. F. THAYER, Cornell University, Ithaca, N. Y.

PHOTOGRAPHIC REPRODUCTION OF COLOR. By ROMYN HITCHCOCK, New York, N. Y.

SIXTEENTH ANNUAL REPORT OF THE COMMITTEE ON INDEXING CHEMICAL LITERATURE. By H. CARRINGTON BOLTON, Chairman, Washington, D. C.

THE Committee on Indexing Chemical Literature respectfully presents to the Chemical Section its sixteenth annual report, covering the twelve months ending August, 1898.

WORKS PUBLISHED.

A Bibliography of the Metals of the Platinum Group, Platinum, Palladium, Iridium, Rhodium, Osmium, Ruthenium, 1748-1896. By Jas. Lewis Howe, Smithsonian Miscellaneous Collections, 1084. City of Washington, 1897. Pp. 318, 8vo.

This fine volume forms one of the most valuable and comprehensive indexes to an important field of chemical literature produced under the

auspices of the Committee since its appointment in 1882. It shows on every page evidence of conscientious and critical skill; the author- and subject-indexes, with which the book concludes, are important features. Its workmanship and the method of presentation of data in type make Dr. Howe's volume a model.

References to the Literature of the Sugar-Beet, exclusive of works in foreign languages. By Claribel Ruth Barnett. U. S. Department of Agriculture. Library Bulletin, June, 1897. 9 pp., 4to.

This carefully edited contribution to the bibliography of a subject interesting to the chemist as well as to the scientific farmer, manifests the activity of the U. S. Department of Agriculture in its Library.

A Chemical Bibliography of Morphine, 1875–1896. By H. E. Brown, under the direction of A. B. Prescott. Completed in Pharmaceutical Archives, Vol. I, No. 3.

A supplement carries the literature through 1897. The separates contain an index of authors.

A Bibliography of the Metallography of Iron and Steel. By Albert Ladd Colby. Published in the Metallographist, Vol. I, No. 2, pp. 168–178. April, 1898.

This includes 188 titles, arranged alphabetically by authors, and numbered chronologically; it is reprinted, extended and re-arranged from The Iron Age, January 27, 1898, by the author.

Review and Bibliography of the Metallic Carbides. By J. A. Mathews. Smithsonian Miscellaneous Collections No. 1090. City of Washington, 1898. pp. 32. 8vo.

Published by the Smithsonian Institution on recommendation of this Committee. Under each metal forming a carbide the author has given a brief synopsis of the chemical data with references to the literature on that subject. There is an author-index.

A Catalogue of Scientific and Technical Periodicals 1665–1895, together with Chronological Tables and a Library Check-List, by Henry Carrington Bolton. Second edition. Smithsonian Miscellaneous Collections No. 1076. City of Washington. 1897. pp. 1247. 8vo.

This bibliography was first issued in 1885 and the second edition has been prepared at the request of the Smithsonian Institution. It embraces periodicals in every department of pure and applied science, including of course chemistry and chemical technology. The Chronological Tables give the date of publication of each volume of about 550 periodicals; they enable one to ascertain the date of a given volume in a given series of a given work, or to determine the number of a volume when the date only is given. The Library Check-List shows in what American Libraries, 133 in number, complete sets of the periodicals may be found. The second edition brings the date down to 1895; its publication has been delayed by the compilation of the Check-List. The periodicals catalogued number 8603.

Alkaloidal Estimation; a bibliographical index of chemical research prepared from original literature for the Committee of Revision and Publication of the Pharmacopœia of the United States of America, 1890–1900, by Paul I. Murrill under the direction of Albert B. Prescott. Ann Arbor, 1898.

A pamphlet of about sixty pages, not for sale.

The *Review of American Chemical Research* edited by Arthur A. Noyes and published in the Journal of the American Chemical Society completed Vol. III in December 1897. Two indexes, an author- and a subject-index, increase its value.

Mr. E. W. Allen, Acting Director of the Office of Experiment Stations, U. S. Department of Agriculture, in reply to inquiries, sends the following communication.

"During the past year we have completed Volume IX of the Experiment Station Record (1897–'98). This, like former volumes, contains abstracts of and references to articles on the methods and results of work in agricultural chemistry published in this country and in Europe. During the past year the review of Russian scientific periodicals has been added. About 2,000 cards of the Card Index of Experiment Station Literature have been issued during the past year, making a total of 16,000 cards in this index at present. In addition to several accounts of chemical studies of the nutrition of man, the Office has issued a "Report of preliminary investigations on the metabolism of nitrogen and carbon in the human organism with a respiration calorimeter of special construction," by W. O. Atwater, C. D. Woods, and F. C Benedict, the work being carried on in part by funds furnished by this Office. In this connection may also be mentioned a compilation of over 400 pages of metabolism experiments in which the balance of income and outgo was determined, made by W. O. Atwater and C. F. Langworthy, and issued from this Office. The bulletin is a digest of about 3,600 experiments with man and animals in which the balance of one or more of the factors of income and outgo was determined. While this work may at first thought seem somewhat a field, it is chemical in its character as well as physiological and has involved in some cases quite extensive chemical studies."

The "Digest of Metabolism Experiments" herein named forms Bulletin No. 45 of the Office of Experiment Stations, and constitutes a comprehensive bibliography of the subject.

The Committee chronicles the publication of the following bibliographies bearing more or less on chemical researches:—

Contributions to the Bibliography of Gold. By A. Liversidge. Australasian Association for the Advancement of Science. Brisbane, 1895, 8vo.

Analyst (The). The organ of the Society of Public Analysts. *General-Index* to the Proceedings of the Society of Public Analysts, Vol. I (1876) and to the Analyst Vols. I–XX (1877–1896). Compiled by J. Cuthbert Welch. London, 1897. Pp. i–181, 8vo.

Arranged on the dictionary plan, authors, subjects and cross-references in a single alphabet.

Bibliographie des travaux scientifiques (sciences mathématiques, physiques et naturelles) publiés par les Sociétés Savantes de la France. Par J. Deniker. Tome I, part 2. Paris, 1897, 4to.

Part I was published in 1895. Most important for original papers published in France.

Biographisch-Literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften, von J. C. Poggendorff. Dritter Band (die Jahre 1858 bis 1883 umfassend). Herausgegeben von B. W. Feddersen und A. J. von Oettingen. Leipzig, 1897-98. Roy 8vo.

This valuable addition to Poggendorff's well-known biographical dictionary is useful to chemists inasmuch as it gives the titles of original papers by the principal chemists of the world published during the period specified.

WORKS IN PREPARATION AND REPORTS OF PROGRESS.

A subject- and author-index to the first twenty volumes of the *Journal of the American Chemical Society* is being compiled by Mr. Sohon, who expects to complete the MS. in December, 1898.

A First Supplement to the Select Bibliography of Chemistry, 1492–1897, by Henry Carrington Bolton, is now going through the press. It will form a volume of the Smithsonian Miscellaneous Collections of about 600 pages.

A *Second Supplement* to the same bibliography, to contain chemical dissertations only, is well advanced, about 8000 titles being already in hand.

Dr. A. C. Langmuir reports the completion of his MS. *Index to the Literature of Zirconium.*

Dr. C. H. Jouet reports the near completion of his *Index to the Literature of Thorium.*

Mr. George Wagner reports progress on a *Bibliography of Oxygen.*

The manuscript of an *Index to the Literature of Thallium* 1861–1896, by Miss Martha Doan, lately of Cornell University, was submitted to the Committee, through Prof. L. M. Dennis, and after critical examination it has been unanimously recommended to the Smithsonian Institution for publication.

Dr. Alfred Tuckerman is engaged upon new editions of his *Indexes to the Literature of the Spectroscope*, and of *Thermodynamics*, which are to be continued to the year 1900. He also reports progress on the *Index to the Mineral Waters of the World*, the printing of which has been delayed by mechanical difficulties.

Dr. Willem P. Jorissen, of Rotterdam, has undertaken to bring down to date Prof. Albert R. Leeds' *Indexes to Ozone and to Hydrogen Peroxide*, first issued in 1880, and long since out of print.

Monsieur Jules Garçon, chemical engineer (of 40 *bis* Rue Fabert, Paris), is about to publish an important contribution to the bibliography of technical chemistry, entitled: "Répertoire universel de bibliographie des industries tinctoriales et des industries annexes." It is expected to form three large volumes. In the preparation of this immense undertaking the author has examined 1800 works and 111 sets of periodicals, the latter in 5000 volumes, besides 7000 other articles and documents. Subscriptions (100 francs) may be sent to the publishers, Gauthier-Villars et fils, Paris. M Garçon is known as the author of the "Bibliographie de la technologie chimique des fibres textiles," Paris, 1893, noted in our Thirteenth Annual Report.

Two unfinished manuscript indexes are at the disposal of persons willing to undertake their completion: an *Index to the Literature of Carbonic Oxid*, begun by the late Prof. William Ripley Nichols and continued by Prof. Augustus H. Gill; and an *Index to the Literature of Milk*, begun by Prof. Clement W. Andrews.

As stated in previous reports this Committee does not attempt to prescribe a fixed plan for volunteer indexers, but leaves method and topic to be chosen by compilers; the committee does not seek to control the productions further than to insure work of high merit and to guard the interests of the Smithsonian Institution which has agreed to publish manuscripts endorsed by the Committee. Chemists willing to undertake the compilation of indexes are requested to send their names and addresses with a memorandum of the subject chosen to the chairman of the Committee (Cosmos Club, Washington, D. C), who will furnish sample copies of indexes and other information.

Committee, { H. Carrington Bolton, *Chairman*,
F. W. Clarke,
A. R. Leeds,
A. B. Prescott,
Alfred Tuckerman,
H. W. Wiley.

American research in physiological chemistry. By E. E. Smith, New York, N. Y.

The time during which chemistry has been applied in physiological research corresponds, practically, to the lifetime of this Association. This period has been christened the era of positiveness. It was inaugurated in

1842 when Meyer pointed out the physiological relations of the brilliant discoveries of Joule, Grove and Helmholtz in the domain of physics. Since then, physical methods of investigation have been applied to the interpretation of vital phenomena. As you know, it was the fortune of the eminent von Liebig to investigate this new and important field and his "Animal Chemistry," edited for the American profession in 1842 by Prof. John W. Webster of Harvard University is, I believe, the first important publication in America on physiological chemistry.

The subject of physiology had not acquired the dignity of a special department of learning. It was taught by men of general education; in the medical school, usually by a physician engaged in the practice of his profession. So, likewise, the earlier American investigations in physiological and pathological chemistry were at the hands of general chemists, physicians and agriculturists, men whose names in most instances are more prominently associated with other departments of science. It is only during the present generation of teachers and investigators that the names of a few American scientists have become primarily associated with physiological chemistry.

In noticing the work of the early American investigators, let me first call your attention to the observations on gastric juice and the physiology of stomach digestion, made by Dr. Beaumont, Surgeon of the United States Army. Alexis St. Martin, a Canadian, was the subject of these observations. This individual developed a gastric fistula from a gun-shot wound. The peculiar condition afforded Beaumont the opportunity, in 1825, to obtain for the first time, from the human subject, pure normal gastric juice, and to study the chemistry and physiology of gastric digestion. The last observations on St. Martin were published in 1856. The case is memorable in the history of physiology.

The malarial fevers were at this time prevailing in many localities. Curiously enough the etiology of this form of disease was studied from a chemical point of view, a fact which explains the publication of such articles as "The Influence of Vegetable and Animal Decomposition as a cause of Fever," "The Relation of the Dew Point to the Diseases of Marshy Lands" and "On the Active Principles of Malaria." In this last article, Professor Gardner presents the theory that sulphuretted hydrogen is the active agent in the production of the disease, basing this claim upon the observation that malarial infection was common in swampy regions. The instance was cited by his opponents that Boston, though swampy, was free from the disease. This opposition Gardner met by the ingenious explanation that Boston owed its exemption from malaria to the presence of metal in the soil, which formed insoluble sulphides with the sulphuretted hydrogen.

Liebig's materialistic views were entirely too radical to find universal acceptance at the time of their appearance, even among professional men. Accordingly we notice, in the literature of the few years following, vigorous protests to his teaching of the identity of vital and chemical force. Dr. Caldwell of Louisville ridiculed calling the body a "corporeal stove

for burning oxygen." The literature of this time contains articles by John W. Draper, M.D., although his "Human Physiology" was not published until 1856. In 1837 he wrote "On the Physical Action of Capillary Systems," calling attention to the identity of "the force producing motion with the chemical force," an idea which he further discussed in 1846 under the title "Circulation of Blood as depending on Chemical Action." Both Draper and Gardner contributed to the development of our knowledge of the relations of light to vegetation.

The subject of animal heat attracted the attention of physiologists in the forties. The theory of its origin, now universally accepted, was receiving the critical attention of those interested in this phase of physiology. One writer, not satisfied with this prevailing theory that it is "due to decarbonization of the blood," contended that if oxidation occurs in the lungs, this organ must necessarily be hotter than other parts of the body. He held that animal heat is evolved in the change from fluids to solids which occurs in the deposit of new matter from the blood to replace body waste.

Beaumont's important contributions on stomach digestion previous to 1850 were followed, both at home and abroad, by studies of the chemistry of the entire digestive process. In this country such investigations, during the third quarter of the century, by the prominent physiologist Prof. John C Dalton, are particularly deserving of notice This investigator contended that the conversion of carbohydrates into sugar is a property of intestinal rather than gastric digestion, and also studied the composition and physiology of bile and questions connected with the glycogenic function of the liver. An important discovery in this field was announced in 1862 by the well-known physiologist Prof. Austin Flint, who stated that there is normally present in human feces a cholesterin-like substance called by him "stercorine." This substance is, indeed, a product formed from the cholesterin of bile during its passage through the intestines. It is interesting to note that two German investigators have recently confirmed this discovery of Flint and announce that stercorin differs from cholesterin in containing two more atoms of hydrogen.

There has developed in this country in the present time, a physiological inquiry, the value and importance of which I believe few of you fully realize. I refer to the investigations on the food and nutrition of man pursued with the aid and under the supervision of our Government. In order to give you even a superficial idea of these studies. I must briefly call attention to the factors which have combined to stimulate so important an inquiry.

The history of American investigations on the food and nutrition of man is first individual and personal. Before the present generation, occasional and scattered contributions appeared from investigators whose interests and departments of work merely touched at this point. In 1869, came the publication of an essay on the composition of American maize by W. O. Atwater, then a student in the laboratory of Professor Johnson, at Yale College. No one, least of all probably Professor At-

water, realized at that time that this was the inception of an inquiry destined to reach such proportions as to invite the coöperation, under government supervision, of scientific men scattered throughout the country. How this came about is intimately associated with the establishment and development of our Agricultural Experiment Stations.

While I do not purpose here to review work in agriculture I will yet briefly refer to Professor Atwater's investigations from the point of view of physiological chemistry. This investigator, just a quarter of a century ago, in an address before the Maine State Board of Agriculture, made what I believe is the first public presentation in the English tongue of the modern theory of nutrition of animals and of the functions of nutrients. Corresponding statements, bearing on the food and nutrition of man, were made by the same author in a paper on the chemistry of foods in 1885. Professor Atwater's conceptions of the process of nutrition were such as to suggest to him the desirability of extensive and accurate studies of food materials and food consumption and his early scientific contributions furnish the beginning of such inquiries in this country.

The earlier work of the agricultural experiment stations was directed almost exclusively to agricultural production. It became apparent, however, that the interests of the community which this organization serves demanded a broader field of inquiry; that the raising of crops bears an important relation to food economy and food consumption. Aid was, therefore, afforded to investigations on the nutrition of man carried on under the supervision of the department of experiment stations. The immediate direction of such nutrition investigations was given to Professor Atwater. Through this channel, a department of study first pursued by this investigator as an individual has become the field of investigation of a large number of scientists throughout the country. Thus a stimulus has been given to research on the chemistry of nutrition, and the coöperation of widely scattered investigators has been brought about, with evident benefit to the individual workers and to the work itself.

It would be interesting to present to you statistics of the vast number of food analyses made in connection with these studies, but I can scarcely do more than mention some of the more purely physiological features of the work. Food consumption has been studied by the direct analyses of dietaries of people of different occupations in various localities, both North and South; food economy has been studied by direct observations of the cost of these dietaries in relation to their nutritive values; digestibility has been determined by observations of the amount and character of the food which actually remained undigested. The conclusion is reached that our national dietary is one-sided, containing much larger proportions of fuel materials, fats and carbohydrates, than the best evidence implies to be appropriate. This appears to result directly from the fact that the food production of the United States is out of balance, containing an excess of these same materials.

Of even greater interest to physiologists are the studies of the laws of

nutrition which are being pursued as a part of these same investigations, including studies of the metabolism of matter and energy. To accomplish what is intended in these inquiries has necessitated the expenditure of much time and effort upon the apparatus used, notably upon a bomb calorimeter and a large respiratory calorimeter. I regret that our time does not permit us to review the interesting conclusions already reached as a result of these investigations.

Of the various problems that physiological chemists have studied in recent years, none are of greater interest and importance or have taxed the care and ability of the investigators more, than the study of the group of proteids. The imperfect methods used in the separation of these complex substances, even in the investigations of no longer than two decades ago, yielded such impure and modified products as to render their study confusing. What has been done at the present time is to develop new methods of separation, notably by the use of various saline solvents and precipitants, and to use more accurate methods of analysis in the study of chemical composition. Much, too, has been accomplished by a study of the cleavage products formed by the action of chemicals and enzymes. Investigation on the native proteids and their cleavage products made by Chittenden during the last fifteen years are a most important contribution to our present knowledge of these substances. Osborne, working first with Chittenden in a study of the proteids of maize, and later independently and with a number of co-workers, has made an extensive and most valuable study of the proteids of the vegetable kingdom, isolating these substances, studying their chemical properties and determining their composition. His investigations have led to a new nomenclature of vegetable proteids and are yielding most interesting conclusions in regard to the physiology of germination and plant growth in general. Of especial interest at this time are Osborne's studies of the crystallization of vegetable proteids, particularly when considered in connection with similar work on the animal proteids by Hofmeister and other European investigators.

While something was accomplished by the study of the chemistry of digestion during the second and third quarters of the century, much yet remained to be revealed by the more definite and accurate methods of research of the present time. In this field the work of Professor Chittenden, particularly his investigations of the products formed in the digestion of proteids, are among the foremost contributions to the literature of physiological chemistry. Since the identification and classification by Kühne and Chittenden, of the products formed in the digestion of albumin, Chittenden, aided by various co-workers, has extended our knowledge of digestive proteolysis by the study of every important proteid. The same observer has studied the influence of various substances and conditions on enzyme action and on the more purely physiological phenomena of digestion, contributing to our knowledge in this department of physiology a chapter of much practical value.

Among the earliest American experiments on metabolism are those by

Flint in 1873 with a professional pedestrian during vigorous exercise. He concluded that severe muscular exertion increases the excretion of urea. Similar observations by Jones in New Orleans in 1878 led to the same conclusion. Of later investigations, those by Chittenden on the influence of various substances on proteid metabolism constitute important contributions. The work of Atwater has already been mentioned. Lusk has recently shown that following the administration of phlorhizin to starving dogs, no less than 60 grams of dextrose may be derived in metabolism from 100 grams of proteid, thus indicating what a considerable portion of the proteid molecule is non-nitrogenous. It is the recognition of this fact by Lilienfeld and the introduction by him of the carbohydrate radicle through the use of formic aldehyde that have contributed such a degree of success to his attempts to produce proteid by synthesis.

Physiological chemistry has rendered important service, as you know, in the recent growth and development of the science of Bacteriology. In this country, Vaughan in 1885 succeeded in isolating an active agent from poisonous cheese, to which he gave the name "tyrotoxicon." In 1890 Novy and de Schweinitz, working independently, separated both basic and non-basic poisons from cultures of the hog-chorera bacillus. Vaughan in the same year reported toxins in cultures of three toxicogenic germs found in drinking water, also in cultures of three of Booker's summer diarrhœa germs and in poisonous cheese.

In 1888, Vaughan and Novy published a small, but excellent treatise on "Ptomaines and Leucomaines," a work which may be regarded as the first attempt to collate everything of importance on these subjects. The third edition of this book, which appeared in 1896, shows an increase in size to over six hundred pages. There is no better index, than this, to the growth of modern research in this department of chemistry.

By the announcements of Koch of the discoveries of the tubercle bacillus and of tuberculin, great interest was aroused in the chemistry of this organism and its products. To this subject contributions of great importance have been made by the scientific investigations of Dr. de Schweinitz in the Bio-Chemic Laboratory of the Bureau of Animal Industry at Washington. Prudden suggested, in 1892, that caseation, so characteristic of tubercular degeneration, is probably due to a specific metabolic product of the tubercle bacillus. De Schweinitz has separated and studied this chemical substance. The fact that the dried germs contain 37% of fat was announced by de Schweinitz and Dorset in 1895. Following this came the interesting observation that the ether extract contains a high melting acid which on analysis yields a formula corresponding closely to teraconic acid, $C_7H_{10}O_4$, an unsaturated acid of the fatty series and that the injection of the product into the liver of animals is followed by the formation of necrotic areas. These discoveries suffice to indicate the progress and value of chemical research in bacteriology.

During the past few years studies of the chemical factors causing certain diseases have been in progress in this country, researches which were stimulated at the outset. perhaps, by the claims of an English phy-

sician that a long list of complaints are caused by an increase of uric acid in the blood. Herter has concluded that in these conditions uric acid is a result rather than a cause of disease and emphasizes the importance, as etiological factors, of the products of fermentation and putrefaction absorbed from the alimentary canal. Rachford presents evidence to show that the symptoms of toxicity are an expression of leucomaine poisoning, dependent on defective elimination. The studies of Herter on the toxic factors in the uræmic states also constitute an interesting and important chapter in American research in pathological chemistry. The growing importance of this field of inquiry is indicated by the fact that in the organization of the Institute of Pathology of the New York State Commission of Lunacy, provision has been made for research in chemical pathology and also by the recent establishment of a professorship in pathological chemistry by the University and Bellevue Hospital Medical College of New York. In the Pathological Institute, Drs. Levene and Bookman, the Associates in Physiological Chemistry, have studied the toxicity of the blood and urine in various pathological conditions and the acidity and alkalinity of the body tissues.

American contributions in physiological chemistry are found scattered through the literature of chemistry, physiology, medicine and general science, in publications at home and abroad, in the English and foreign tongues. The increasing number of scientific investigations has long demanded special facilities for publication. This demand has now been met by the establishment of two American periodicals, viz.: "The Journal of Experimental Medicine," and "The American Journal of Physiology." In the two completed volumes of the Journal of Experimental Medicine, twelve articles record results of chemical investigations. They include contributions by Aldrich on the occurrence of unoxidized sulphur compounds; a study by Abel of the pigment of the negro; contributions from the laboratories of the Harvard Medical School on the composition of human bile and on the clotting of milk; and studies from the laboratories of Bryn Mawr College on the digestive enzymes, besides research on subjects and from sources already mentioned. The single published volume of the American Journal of Physiology contains the results of seven chemical investigations, including contributions by Mendel on edible American fungi and an article by Mathews on the chemistry of cytological staining. Nearly one-half of the total number of chemical investigations recorded in the two periodicals are contributions from the laboratories at Yale University. This fact is a fair index to the activity at this institution in this branch of scientific research.

I cannot close without a word in recognition of foreign scientists. American research has been collateral with the general progress of physiological chemistry. While loth to admit that American investigators have altogether followed in the wake of European masters, I yet recognize their considerable influence on American research, particularly the influence of German scientists. It is not necessary to apologize for this. The development of science and art in a country necessarily proceeds

slower than civil and industrial progress. It is fair to believe, however, while still recognizing the unity of purpose in scientific work, that at the present day American research in physiological chemistry and in the sciences generally, has gained a spirit of independence and is establishing at home and abroad a reputation worthy of the country whose industrial and civil progress has so amazed the world.

STUDIES IN EPILEPSY: A CONTRIBUTION TO THE METABOLISM IN NERVOUS DISEASES. By S. BOOKMAN, M.A., Ph.D., Associate in Physiological Chemistry at the Pathological Institute of The New York State Hospitals.

THIS abstract is a type of the investigations carried on along these lines, on several cases of idiopathic epilepsy. The fluids studied were the urine, blood and stomach contents.

The diet of the patient had been carefully observed, the fluids being measured and the solids weighed for each meal. A twenty-four hour sample of the urine was obtained each day, and the blood and the stomach contents once each week. The following quantitative determinations were made: Total solids, inorganic solids, as sulphates, urea, uric acid, total nitrogen, total phosphoric acid, preformed sulphates, ethereal sulphates, chlorides, ammonia, potash and soda. The specific gravity was carefully determined and all percentages were based upon the weight of urine taken.

Further than this the toxicity of the urine was determined for each twenty-four hour sample, by injecting the same into the circulation of a rabbit until death ensued.

The blood and the gastric contents were obtained once each week, Ewald's breakfast being administered on an empty stomach one hour previous to taking the latter.

Having obtained the various percentage present in the urine, a table was constructed containing the amounts present in each twenty-four hour sample, and another table the amounts of the various constituents in the toxic dose. It will readily be seen how little reveals itself in studying a table of figures. It occurred to me to plot *curves to scale*, of the various constituents determined. By drawing a perpendicular dotted line for each convulsion we see at a glance what has happened during each cycle and by comparing the different curves we are enabled to study the metabolic changes in a most comprehensive manner.

Thus the amounts of urine and the specific gravities vary inversely, the rise of one being accompanied by a fall in the other. Total solids fall during the twenty-four hours following a convulsion, following the curves of the specific gravity, inorganic solids follow in a similar course, as also do the urea and nitrogen. The ammonia seems to diminish prior to a convulsion, and to rise during the following twenty-four hours; the uric acid, however, seems to reach a maximum midway between each

convulsion, the amount excreted reaching almost a minimum after each seizure.

Total phosphates increase during the twenty-four hours following each convulsion, indicating the destruction of nerve tissue.

Our curves further show us that after a series of two or three convulsions, there is a marked increase of ethereal compared to preformed sulphates.

Three or four days prior to each convulsion the ratio of uric acid to phosphoric acid approaches to 1 : 1½, the condition prevalent in uric acid diathesis.

We find a marked fall of chlorides immediately after each convulsion, the total nitrogen as well falling slightly below the normal at the same time.

The toxicity of epileptic urine varies from 61 to 305 ccm. per kilogram of rabbit, in the case here cited, in every instance the toxicity diminishing before a convulsion to rise after the convulsion had ceased.

The toxicity of the blood serum ranges from 17.2 to 42 ccm. per kg.

The amount of free acid in the gastric contents reached a maximum several days after a convulsion, and falls to a minimum immediately after the seizure.

A more detailed treatise on these investigations will appear in an early issue of the Archives of Neurology and Psycho-Pathology.

INVESTIGATIONS ON THE NATURE OF THE NISSL GRANULES. By S. BOOKMAN, M.A., Ph.D., Associate in Physiological Chemistry at the Pathological Institute of the New York State Hospitals.

THE Nissl bodies or granules obtained by staining a precipitated constituent of the cytolymph of the nerve cell according to the Nissl methylene blue method (Neurologisches Centralblatt 1894, p. 507) are fine or coarse flocs found in the nerve cell in smaller or larger proportions according to the condition of their precipitation. They are probably in solution in the cytolymph or cell sap, and are either a catabolic or anabolic derivative of the same. In short, they are a constituent of the normal cytoplasm. Held has found that these bodies are not present in the living cell, but precipitate in less than thirty minutes after death. My experiments on the changes of the acidity and alkalinity (Proceedings of the N. Y. Pathological Society, 1898) have further substantiated these results. It is most likely that there exist in a pathological state of the body conditions analogous to those which produce the Nissl bodies in post-mortem material, viz.: the gradual rise in the acidity of the lymph or serum reducing the natural, slightly alkaline reaction of these fluids.

On this property of precipitating the Nissl granules with acid, I based the method of obtaining the same from nerve tissue.

Preparation. A large number of sheep's brains were quickly removed from the skull containing them, freed from membranes and blood vessels,

finely macerated and quickly put into 0.75% sodium chloride, which corresponds to a physiological salt solution, containing sodium chloride in about the same proportion as ordinary blood serum. These are approximately the conditions present in the body, and as the material becomes gradually more acid after death, the Nissl granules precipitate. The material was removed and put into the fluid within fifteen minutes after death. I controlled the acidity by making determinations from day to day until I found the acidity to have reached a maximum. This mass was filtered, the filtrate being carefully neutralized with 1% sodium hydrate and filtered again. The Nissl bodies being soluble in dilute alkalies, they were found in the filtrate. By reprecipitating with dilute hydrochloric acid, filtering, extracting with cold ether to remove any fat or fatty acids, redissolving the residue in dilute soda and reprecipitating with dilute acid, a light flocculent precipitate was obtained, which on drying in vacuum presented a slightly yellowish tinted, amorphous powder soluble in dilute alkalies and lithium carbonate, and reprecipitated from this solution by dilute acids.

These bodies in all probability belong to the group of nucleo-proteids, their reactions pointing in that direction.

The chemical composition, ultimate analysis and coagulation point, as well as further research on these granules, will appear in the Archives of Neurology and Psycho-Pathology at some later date.

PROTEIDS OF THE BRAIN. By P. A. LEVENE, Pathological Institute of the N. Y. State Commission in Lunacy, New York, N. Y.

THE author describes the separation by recent methods of the proteids, nucleo-proteids and nucleins in brain tissue, their identification and composition.

EXPERIMENTS ON THE METABOLISM OF MATTER AND ENERGY IN THE HUMAN BODY. By W. O. ATWATER and F. G. BENEDICT, Middletown, Conn.

EXPERIMENTS ON THE METABOLISM OF ALCOHOL IN THE HUMAN BODY. By W. O. ATWATER and F. G. BENEDICT, Middletown, Conn.

A DIETARY STUDY OF A BICYCLE RACER. By W. O. ATWATER and A. P. BRYANT, Middletown, Conn.

ON THE AVAILABILITY OF THE NUTRIENTS OF FOOD MATERIALS. By W. O. ATWATER and A. P. BRYANT, Middletown, Conn.

URINARY ACIDIMETRY AND ALKALIMETRY. By HEINRICH STERN.

THE NORMAL DEGREE OF URINARY ACIDITY. By HEINRICH STERN.

ON THE PROPERTIES OF GALACTASE: A PROTEOLYTIC FERMENT OF MILK. By S. M. BABCOCK and H. L. RUSSELL, Wisconsin Experiment Station, Madison, Wis.

THE changes which occur during the ripening of cheese have been generally attributed to the exclusive action of micro-organisms. There are however many phenomena associated with these changes that cannot be satisfactorily explained on this basis. While engaged upon this problem the authors of this paper discovered an inherent proteolytic ferment in milk, to which they have given the name galactase, and which they believe to be the most important agent in converting the insoluble casein of a green cheese into peptones and other soluble proteids found in a ripened cheese. The action of this enzyme is retarded by acids, in which respect it appears to be allied to trypsin. It decomposes hydrogen peroxide rapidly, but loses this power as well as that of digesting casein after having been heated to 80° C. in neutral solutions. It attaches itself to finely divided proteids in suspension, and hence is found in greater abundance in the separator slime and cream than in the skim milk. From the slime, concentrated extracts may be obtained. It is precipitated from its solutions by absolute alcohol, but its activity is thereby diminished. Strong disinfectants destroy it. Chloroform, ether and benzol have been found to be the best agents to prevent bacterial growth and permit the continued activity of this enzyme. It is widely distributed, being found in all milks that could be obtained from different species of animals, including cow, sheep, goat, horse, hog, buffalo, burro and human.

AGRICULTURAL CHEMISTRY.

(Under the leadership of H. A. WEBER, Ohio State University, Columbus, O.)

LIGHT, A FACTOR IN SUGAR PRODUCTION. By H. A. WEBER, Ohio State University, Columbus, O.

THE following propositions in regard to the influence of light on the sugar content of plants were discussed:

1. Plants with a long period of vegetation are not so much affected by

climatic conditions, location, etc., as those having a short period of vegetation.

2. For plants with a short period of vegetation climatic conditions must be taken into consideration.

3. The higher latitudes are more favorable, other things being equal, than lower ones.

4. Proximity to large bodies of water is favorable to sugar production.

5. Topographical features of land are of importance in selecting location of sugar factory.

6. Owing to an over-production of sugar in the world, only such factories can be successful, which have the most favorable locations.

THE DETERMINATION OF STARCH IN AGRICULTURAL PRODUCTS. By J. B. LINDSEY, Amherst, Mass.

A NOTE ON THE GROWTH OF LUPINS ON CALCAREOUS LANDS. By E. W. HILGARD, University of California, Berkeley, Cal.

THIS paper refers to the vegetation experiments made by Heinrich in 1896 showing the sensitiveness of lupins to carbonate or sulphate of calcium in the soil.

Since California is preëminently the country of lupins and nearly all its soils are calcareous, it is evident that different species vary in this regard.

Culture experiments with European lupins in California for green-manure purposes have been unsuccessful, supposedly because of the calcareous nature of the soil; but it has not been shown that by sowing the lupins in autumn they develop well in soil containing fully 1% of calcium carbonate.

SOME OF THE IMPORTANT RESULTS OF THE RECENT CHEMICAL INVESTIGATIONS OF PLANT AND ANIMAL CELLS. By E. A. DE SCHWEINITZ, Biochemical Laboratory, Department Agriculture, Washington, D.C.

THE COMPOSITION OF OHIO WINES. By A. W. SMITH and NORMAN PARKS, Cleveland, Ohio.

IN determining the purity of wines by analytical methods, the main dependence is usually placed on the quantities of solids, ash and alcohol, and on the ratio of alcohol to glycerol. If neither water nor alcohol has

been added, this latter ratio should be one hundred parts of alcohol to from seven to fourteen parts of glycerol, according to numerous analysis of foreign- and California-grown wines. No record being found of analyses of authentically pure Ohio wines, samples were made by the authors personally from ten different varieties of grapes most commonly grown near the Ohio shores of Lake Erie, where the principal part of Ohio wines is made. The methods of analysis were those described in Vol. 43, U. S. Department of Agriculture, Division of Chemistry. All the constituents were found to be similar to the averages of those reported for European and California wines, except solids, ash and glycerol. Solids and ash were very slightly lower than these averages. The main difference was in the ratio of glycerol to alcohol, which ranged from 3.9 to 11.8 parts of glycerol to one hundred parts of alcohol, most samples having about five parts.

These results necessitate caution when judging as to the addition of water or alcohol to Ohio wines by these determinations.

INDUSTRIAL CHEMISTRY.

(Under the leadership of N. W. LORD, Ohio State University, Columbus, O.)

THE VALUATION OF COALS. By N. W. LORD, Ohio State University, Columbus, Ohio.

DETERMINATION OF TURBIDITY IN WATER. By W. P. MASON, Rensselaer Polytechnic Institute, Troy, N. Y.

EFFICIENCY OF THE ELMIRA FILTERING PLANT. By W. P. MASON, Rensselaer Polytechnic Institute, Troy, N. Y.

THE COMPOSITION OF OSCILLATORIA PROLIFICA (GREVILLE), O. RUBESCENS (DE CANDOLLE) AND ITS RELATION TO THE QUALITY OF WATER SUPPLIES. By ISABEL F. HYAMS and ELLEN H. RICHARDS, Mass. Institute Technology, Boston, Mass.

THE increasing use of artificial reservoirs, often made by draining the water level of a small natural pond without previous cleaning, brings a corresponding increase of cases in which disagreeable odors and tastes appear in the water at certain seasons of the year. One of the most difficult questions asked of the water expert is, whether the water so infected is harmful to the human organism; whether a water company under con-

tract to supply wholesome water can be shown to have violated its pledge if the water it furnishes is so full of anabæna or synura or uroglena or any one of the list of odor- or taste-giving organisms, as to preclude its use for drinking and to require courage and faith to use it for bathing during two or three months in the year.

While these organisms make themselves unmistakably known, they are rarely to be had in such abundance or such purity as to admit of the collection of sufficient amount for the ordinary chemical examination. An exception to this rule is found in the plant which makes its home in Jamaica Pond in the suburbs of Boston and the source of the earliest supply for the city (1795) and used until taken for the Park system in 1891.

This plant, belonging to the blue green algæ, named *Oscillatoria rubescens* by De Candolle, appears to be similar to that which was found in Lake of Geneva, Switzerland, in 1834–6 and no record of any appearance elsewhere in like quantity has been found.

To our personal knowledge it showed abundantly in Jamaica Pond in 1886 and has continued until the present time. It makes its appearance in May and lasts until July, when in most years it disappears. During the period of its growth the whole mass of water appears a brownish red, a color like dried blood, and on still mornings after a hot, sunny day, large patches, many square yards in extent, of an orange brown cream, are found floating on the surface. These are composed of the new growth of the day before and soon become distributed into the deeper layers by the wind, or are driven on shore to decay on the rocks, giving to them a rich purple coating, while the surface of the water is covered with a froth, especially around the edges. The odor of growth is a strong grassy odor, not unpleasant, but the odor of decay is intensely disagreeable, fetid rather than putrefactive. In years when the plant is abundant, the odor is evident along the shores of the pond, but as a rule, it is not distinguishable to any but the experienced nose. The fact that this plant occurs as an almost pure culture, and when taken from the patches of cream may be used directly for experiment, renders it peculiarly fitted for examination chemically.

It contains:—

Nitrogen	S	P	CaO	MgO	Iron	Ash
11%	.70	.47	.35%	.13%	present	3.37%

It is evident that the products of decomposition have a good right to yield odors. Whether these substances are harmful in these small quantities, remains to be proved. Reasoning from analogy, we should expect to see effects only from long continued use; certainly we have experienced no ill effects from the rather large doses which we have taken in the course of the examination, and during the years of its greatest abundance, 1886 to 1891, the pond was still used as a water supply with no known ill effects. Although we obtained a considerable quantity of the material, the losses at each stage of the process in working on an

entirely unknown substance, left us with only small samples, too small in most cases for purification sufficient to warrant a complete determination of the composition. However, we have succeeded in obtaining from the fresh material, in a more or less pure form:

1. A brilliant blue green oil of exceedingly pungent odor when heated, affecting most painfully the upper portion of the nostril.
2. A yellow green chlorophyll giving the usual spectrum bands.
3. A brilliant red substance containing sulphur and nitrogen, giving the taste and odor of onions, which dyes silk and wool a fine pink color and which we have named rubescine.
4. A substance which crystallizes with a beautiful metallic lustre, soluble in chloroform, ether and acetone to a brilliant orange, and which on oxidation dyes silk and wool a fine yellow. The relation of this to rubescine has not been fully determined.
5. An intensely bitter substance which may not have been completely purified, since it is yet green and dyes silk a delicate green. This we have called oscillarine.
6. A vegetable mucilage similar to that in Irish moss.
7. A nitrogenous substance or vegetable albumen.
8. A saponine or digitonin, which is the substance giving the frothing effect to the water of the pond and which gives a delicate red color to the acid solution containing it.
9. A brilliant purple resulting from slow oxidation of one of the others.

The two reddish coloring matters are obtained only by a modification of the usual plant analysis, namely, extraction of the fresh material without previous drying.

Having determined the habits of the plant and worked out methods for isolating these substances, we now await another season in order to secure a sufficient quantity of fresh material to identify each one fully and to test their physiological effects.

THE LE SEUER ELECTROLYTIC PROCESS FOR THE PRODUCTION OF CAUSTIC SODA AND BLEACHING POWDER. By CHAS. L. PARSONS, Durham, N. H.

REVIEW OF THE ELECTROLYTIC PROCESSES FOR THE PRODUCTION OF CAUSTIC SODA AND BLEACHING POWDER. By CHAS. L. PARSONS, Durham, N. H.

THE ALUM QUESTION IN WATER PURIFICATION. By E. G. SMITH, Beloit, Wis.

THE EFFECT OF AN ELECTROLYTIC BATH ON THE TANNING OF LEATHER. By C. F. MABERY and K. LANDGREBE, Case Scientific School, Cleveland, Ohio.

WE find the time required to tan hides completely is considerably reduced in an electric bath. We also find that the percentage of nitrogen in leather produced in an electric bath is lower than in leather tanned in the ordinary way. Results will be given.

SOME RECORDS OF THE YEAR'S PROGRESS IN APPLIED CHEMISTRY. By WM. MCMURTRIE, New York, N. Y.

THE PROGRESS IN UTILIZATION OF CITY GARBAGE, WITH SPECIAL REFERENCE TO THE NEW PLANT IN BOSTON. By BRUNO TERNE, Philadelphia, Pa.

ON THE REMOVAL OF HARDNESS FROM WATER FOR BOILING PURPOSES. By C. F. MABERY and E. B. BALTZLY, Case Scientific School, Cleveland, Ohio.

WE find that all sorts of hard water, whether caused by calcium carbonate or calcium sulphate, also water with large quantities of suspended matter, have the lime removed to the extent of 90–98% and the suspended matter completely precipitated in the cold after the water has stood twenty-four hours with about half the calculated amount of sodium aluminate, the proportion of the reagent depending somewhat on the composition of the water.

A great variety of waters have been examined with this reagent with excellent results.

A NEW PROCESS FOR THE COMMERCIAL PRODUCTION OF OXYGEN AND ITS INDUSTRIAL APPLICATIONS. By ROMYN HITCHCOCK, New York, N. Y.

ON THE OCCURRENCE OF STRONTIUM AND BARIUM. By J. H KASTLE and W. A. BEATTY, State College of Kentucky, Lexington, Ky.

THE occurrence of the strontium and barium in the limestones of Central Kentucky has been investigated. Altogether about one hundred specimens of limestone have been examined spectroscopically for strontium and

barium:— ten from the Lower Hudson; thirty from the Trenton and fifty from the Birdseye and Chazy limestones from the Kentucky river gorge at High Bridge. Besides these limestones, a number of calcites, aragonites and heavy spars have also been examined; and also a number of the natural waters of this region. Both strontium and barium have been found in all of these limestones and waters,—the former in quantities varying from 0.01473 to 0.07365 per cent of strontium carbonate. The quantities of barium have not yet been determined. The results of this investigation, together with those of Hillebrand on the occurrence of these two elements in the silicate rocks, leave little room for doubt as to the universal association of calcium, strontium and barium in nature. In this connection attention was called to the fact that in general, it would seem, two representative elements of each group occur abundantly in nature the remaining elements of the group being rare.

THE OXIDATION OF FORMIC ALDEHYDE BY HYDROGEN PEROXIDE. By J. H. KASTLE and A. S. LOVENHART, State College of Kentucky, Lexington, Ky.

THE velocity of the oxidation of formic aldehyde by hydrogen peroxide, in aqueous solution, at ½ normal dilution, has been determined for the following temperatures, viz.: 26°C., 30°C., 40°C., 50°C., 60°C., 70°C., 80°C., 90°C., 98°C. At ordinary temperatures the oxidation proceeds with extreme slowness, whereas, at 90° to 98°C. it becomes almost instantaneous. The coëfficients of velocity calculated according to the equation for bimolecular reactions were found to give constant values for any given temperature. It was observed further that light accelerated the oxidation. This acceleration proved to be greatest for lower temperatures, gradually diminishing as the temperature was raised until at 90°C. the coëfficients of velocity in light and dark became the same. The effect of acids and bases on the progress of the reaction was also investigated. Acids were found to produce a slight acceleration on the rate of change, and in the ratio of their strength. Bases were found to accelerate the reaction very greatly. Sodium, potassium and barium hydroxides were found to accelerate to about the same degree, and each of these acted much more strongly than ammonia. It was found further that even alkaline salts greatly accelerated the oxidation, so that the reaction gives promise of being useful in the determination of the hydrolysis of alkaline salts. Further studies along this line are now in progress.

CATALYSIS. By J. H. KASTLE AND J. C. W. FRAZER, State College of Kentucky, Lexington, Ky.

THE numerous theories which have been put forward by different observers were reviewed at some length and some of the difficulties in the way of their acceptance pointed out. As the result of our studies

on the hydrolysis of sulphonic ethers especially we have been led to make the following assumptions regarding the catalytic process:

First, That the catalytic changes involved in etherification, the hydrolysis of ethereal and metallic salts and acid amides, and the inversion of cane sugar and similar compounds by acids, are essentially the same in kind.

Secondly, That hydrolysis and etherification are always preceded by the formation of an unstable addition product of the substance undergoing the catalysis and the acid causing the same; and that it is the decomposition of this unstable addition product by water or alcohol that gives the ultimate products of the catalysis.

Thirdly, That this unstable addition product is produced by the combination of the ions of the acid causing the catalysis with the anhydride or oxide-like oxygen or element of the compound undergoing the change.

In support of these suppositions attention was called to the general conduct of the carbonic and sulphonic ethers. The many catalytic changes produced by sulphuric acid especially become readily intelligible in the light of this hypothesis. Attention was called also to the unstable salts of the carbonic acid amides and to the peculiar acid addition products of alkyl oxides as being examples of the addition products demanded by this theory; and to the fact that hydrolysis is possible in the case of both of these classes of compounds. This hypothesis necessarily involves the idea of the quadrivalence of the oxygen atom, and while the necessity for such a supposition may not be admitted by some, it will be recalled that Brühl reached a similar conclusion from his study of hydrogen peroxide; and as he (Brühl) has already indicated in this connection many difficulties now almost, if not quite, insurmountable would disappear on the assumption that water is an unsaturated compound. In the catalysis of nitrogen compounds there would be involved a change in the valence of the nitrogen atom from three to five. This certainly involves no difficulties; and if not among nitrogen compounds, then why should it among oxygen derivatives, especially when the changes involved are evidently fundamentally related?

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AND

ENGINEERING.

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PAPERS READ.

[ABSTRACTS.]

TIME TESTS OF TIMBER IN COMPRESSION ENDWISE. By Prof. J. B. JOHNSON, Washington University, St. Louis, Mo.

SOME sixty tests were made on dry, long-leaf pine blocks, 1½ in. square and 3 in. long, all cut from the same 2-in. plank. Sticks were first cut out 2 in. square and 48 in. long, and these dressed down to 1½ in. square, and then cut into sixteen pieces each 3 in. long, by a power cross-cut saw. Alternate specimens were then tested in compression endwise in the ordinary manner in a Riehlé testing machine, and the crushing strength determined. The intervening specimens were then loaded with certain percentages of the average strength of the two adjacent specimens, and this load left on till failure occurred, and the time noted. The load was made constant by using a geared Riehlé 30,000 pound testing machine, and mounting the specimen upon a nest of helical car springs which would deform about an inch under the maximum load upon the specimen. The time tests began by putting 95% of the breaking load on the specimen, then 90%, 85%, 80%, and so on. The lowest time load was 65% of the breaking load. There were a number of time tests for each period and when the averages were taken they plotted into a very smooth time curve, from which the following table is taken:

Percentage of crushing load imposed.	90%	80%	75%	70%	65%
Time, in hours, required to produce failure	10 min.	16 hrs.	36 hrs.	72 hrs.	200 hrs.

These tests were made in April and May, 1898, the material having been stored in my testing laboratory for four years, and kept in the laboratory during these tests. To make such a series entirely free from moisture effects, due to changes in the hygrometric conditions of the atmosphere, all the specimens should be shellacked as soon as cut out, to hold them to a constant moisture condition. The above results must be accepted as approximate only, because this was not done.

ON THE GRANULAR COMPOSITION OF PORTLAND CEMENT. By Prof. J. B. JOHNSON, Washington University, St. Louis, Mo.

THE finest sieve which is commercially practical for testing the fineness of cement is one containing about 150 meshes per linear inch. What passes such a sieve is commonly supposed to be an impalpable powder, and to be all active in the setting process. By using the Schöne washing apparatus, with coal oil, the writer separated out finer grades of unground particles of the burned clinker, running from 0.03 mm. to 0.12 mm. in mean diameter, leaving only about forty per cent of the original cement composed of particles having a less mean diameter than say 0.02 mm. By washing the above coarser grades with benzene and drying, and then wetting with water, they showed no tendency to set or harden; whereas the finest portion, when so treated, did harden in a normal manner. This shows that there is probably not over 30 or 40 per cent of ordinary Portland cement which is active in the hardening process. The rest is inert and like so much sand.

A COMBINATION ABSORPTION AND TRANSMISSION DYNAMOMETER FOR LABORATORY PURPOSES. By Prof. JOHN JOSEPH FLATHER, University of Minnesota, Minneapolis, Minn.

IN this paper the author described some recent improvements upon his hydraulic transmission dynamometer, the most notable feature being the employment of a water cooled pulley and a perforated rim which permits the use of a brake band that can be used for absorbing the power furnished to the machine. The fixed pulley, which is ordinarily used to connect the dynamometer to the machine to be driven, has internal flanges for carrying the cooling water, and the rim is furnished with a number of brass bushings drilled through endwise, and provided with a regulating screw by which the amount of water admitted to the brake is readily controlled. The brake band is a three-ply cotton belt seven inches wide connected through two sets of tension springs to the brake regulating screw.

It will be readily understood that the brake band may be quickly removed, thus converting the machine into a regular transmission dynamometer. In either case an automatic record is obtained on a roll of paper which is caused to pass with uniform motion under an indicator pencil that records variations of pressure.

In addition to the force curve traced by the indicator pencil the zero or datum line is traced by another pencil which is adjustable in position and may be located at pleasure on a line parallel with the axis of the shaft. Thus one is enabled to measure the total power required to run a given machine or any subdivided part of it. The two cylinders through which the driving power is transmitted are each ten square inches in area and

are provided with bronze pistons actuated by plunger pins which may be located in either of two positions: the inner for ordinary work; the outer for heavy work, say when it is required to measure 20 H. P., or when the speed of rotation is very slow.

It will be understood that springs of varying degrees of intensity may be used to transmit the pressure from shaft to indicator pencil. These springs are changed as in any ordinary indicator.

The work of friction of the plunger in the cylinder is practically eliminated since the combined area of the plungers is so much greater than in the indicator piston, and since the indicator motion multiplies the distance moved by the indicator piston five times, it will be seen that the motion of the plungers is very small. The more recent machines are provided with adjustable casters which permit the weight of the dynamometer to be carried by wheels or to rest firmly on the floor, as desired.

METHODS OF DETERMINING THE FREQUENCY OF ALTERNATING CURRENTS.

By CARL KINSLEY, Washington University, St. Louis, Mo.

THE accurate determination of the frequency is essential for the correct computation of the relations between electro-motive force, current, self induction and capacity. Since the periodicity must usually be squared it is essential that the determination be made quickly and with but little error.

Dr. John Hopkinson was the first to use a stretched steel wire actuated by an electro-magnet and when the condition of maximum resource was found the periodicity was calculated from the constants of the wire and its tension.

Dr. Campbell (Phil. Mag. Aug., 1896) proposed the use of an iron rod which was thrown into resonance in the same way. From the constants of the rod and its length the periodicity was computed. The rod he found could be more easily used than the wire.

Another method capable of great accuracy and easy manipulation includes the use of a telephone in the alternating current circuit and a resonance tube, by means of which the periodicity may be calculated from equations or it can be gotten from tables already computed (Kinsley, Phil. Mag. April, 1898).

A very satisfactory method will be one which includes the use of a metal strip as the primary resonator whose periodicity may be changed by means of an added weight. An electro-magnet may be used as in the first two cases to set this into vibration. The periodicity can be calculated from the constants of the vibrator (Lord Rayleigh, "Theory of Sound"), or a complete calibration can be made by two determinations. It will usually be more satisfactory to standardize by actually determining the

periodicity of two adjustments of the resonator. The equation to be used for that purpose is

$$t^2 = Dl^3 m + Kl_1^4$$ where

t = time of one complete oscillation,
l = distance of the sliding weight from the support,
m = mass of the slider,
l_1 = length of the vibrating rod.

D and K are the constants of this particular apparatus which may be either calculated or obtained by experiment.

The condition of resonance is obtained with great readiness and the determination may be made with as great accuracy as may be desired.

ON THE EVAPORATION AND SEEPAGE FROM RESERVOIRS. By Prof. L. G. CARPENTER, Fort Collins, Colo.

FROM two seasons' measurements on a series of reservoirs in Colorado, the author determines that in those cases the loss from seepage is much less than from evaporation, amounting to about two feet in depth per annum. Eleven years' determination of evaporation shows the loss from that source to be forty-one inches from his standard tank. From the reservoirs under observation, several years' record indicates a loss of about sixty inches from evaporation.

[This paper is printed as Bulletin No. 45, Colorado Agricultural Experiment Station.]

LOSSES FROM RIVERS. By Prof. L. G. CARPENTER, Fort Collins, Colo.

THE author reports the results of a series of measurements made on a number of rivers in Colorado. Two measurements were made on the Rio Grande del Norte over a stretch of eighty miles; fourteen measurements on the Cache a la Poudre for forty miles; one on the Arkansas for two hundred miles; one on forty miles of the big Thompson; seven on the South Platte for one hundred and fifty miles; a distance of over two thousand miles gone over in detail, and on which all water taken out by ditches or entering through tributaries was measured.

While gains from seepage is the rule, there are sections on nearly all of these streams where losses are found year after year and sometimes to a considerable extent. The Rio Grande loses from seventy to one hundred cubic feet per second in a distance of ten miles. On the other streams the loss is less. In the case of the Platte there is some reason to think

that a portion of the water reappears. In the case of the Rio Grande, neither the water thus disappearing from the river, nor the water sinking in the numerous small streams, reappears within Colorado.

ON THE TESTING OF STEAM-PIPE COVERS. By CHAS. L. NORTON, Boston, Mass.

THE manifest economy attendant upon the use of some sort of a non-conducting cover to prevent loss of heat from a hot steam pipe has led to many tests of the relative efficiency of materials in use for this purpose. There is a considerable discrepancy between the reported tests upon the efficiency of the materials in most common use, and there are some new materials used for this purpose. The facts seemed to warrant further investigation of the subject.

A series of tests has been made by means of an apparatus devised by the writer. A piece of steam pipe was heated electrically, by means of a coil of wire in oil within the pipe.

The amount of energy necessary to keep the pipe at a definite temperature is measured. Since the energy supplied is just sufficient to keep the pipe at a constant temperature it is therefore just equal to the energy lost by the pipe. Hence from the electrical energy supplied we may compute the heat lost from the outside of the pipe. The economy of the use of some of the better covers is computed, and further an examination of the fire-resisting properties of the several covers is made.

HIGH SPEED INFLUENCE MACHINES. By CHAS. F. WARNER, Cambridge, Mass.

THE causes of the electro-motive force in influence machines, their reversals and certain other peculiarities are not known; hence improvements have been effected by purely experimental means. A reference to some properties of earlier machines is made and to recent improvements under patents by Waite, Myers and Rose, Holopeter and Elihu Thompson.

The special improvements which it is the main purpose of this paper to recount are effected by special insulation; improved contacts, careful balancing of revolving parts and peculiar driving mechanism. The inconvenience from reversals is largely overcome by a newly-devised commutator. In order to provide for discharges of great frequency, such as are required, for example, to produce the Roentgen rays effectively, the principal aim has been to secure high speed with ease and safety. Two forms have resulted: one, designed for compactness, has ball bearings of the highest grade; the other, of larger dimensions, has gun-metal bearings. The speed attained exceeds 4,000 revolutions per minute, which is the

highest rate I have been able to find recorded for influence machines. The resulting discharges are of great frequency and, for certain purposes, are remarkably efficient.

A DYNAMOMETER FOR MEASURING THE WORK DONE IN PULLING A TRAIN. By Prof. THOMAS GRAY, Terre Haute, Ind.

THIS is a spring dynamometer provided with attachments for integrating the total work done during any particular run and also with autographic appliances for recording the pull at any instant during the run. The peculiarity of the spring part is that the springs are held in sufficient compression, by means of a strong steel frame, to allow the pull to be applied at the middle of the spring. In this way one part of the spring is relieved of compression while the other part has its compression increased. The object is to provide definiteness of zero and constancy of scale from the beginning of the pull. The pull is applied to one end of the steel frame and to a stiff plate introduced into the middle of the spring system. The middle plate is, relatively to the frame of the dynamometer, the movable plate. This plate carries, by means of an arm attached to it, the integrating mechanism. This consists of a revolution counter provided with a friction wheel intended to roll on the surface of a flat disk which turns with a speed proportional to that of the car. When the pull is zero the friction wheel is adjusted so that rotating the disk does not turn it. Afterwards the rate of revolution is proportional to the product of the pull on the spring and the speed of the car. When a record of the pull at any instant is desired the arm which carries the counting mechanism, or a separate one, is provided with a pen which is arranged to write on a band of paper fed forward by the motion of the car.

[This paper is published in full in the Railroad Gazette.]

INSTRUMENTS AND METHODS OF HYDROGRAPHIC MEASUREMENTS. By F. H. NEWELL, U. S. Geological Survey, Washington, D. C.

BY the organic law creating the Geological Survey, the Director is charged with the duty of the classification of the public lands and examination of the geological structure, mineral resources, and products of the national domain. Under this general law extensive surveys have been carried on through nearly nineteen years. It has been found that throughout a great part of the country the most valuable of the natural resources is the water. Especially is this true of the western half of the United States where all land values rest upon questions of water supply. The subject of the hydrography of the country was thus early forced upon the

attention of the topographers and geologists of the survey and a large amount of information collected as incidental to other work. In 1888 the division of hydrography was created, systematizing the observations and data bearing upon the water resources and examinations were begun of the distribution and fluctuations of waters above and under ground. The methods employed have resulted from many years of experience and are of a character such as to obtain the desired information with an accuracy consistent with a fair degree of scientific value, and yet with full consideration of the expenditure available.

In the measurement of various streams, especially of the larger rivers, it has been found desirable to seek a locality where the bed and banks are fairly stable and the velocity of the water moderate. The width and depth have been obtained by direct measurements and the rate of flow determined at various points in the cross sections by means of various forms of current meters. Computations based upon the slope and other factors have been made in a few instances, but mainly as a matter of curiosity to note the possible accordance with the results of detailed current meter measurements. Estimates of the flow over weirs or dams have been had occasionally, but the uncertainties involved in the leakage and discharge through gates, water wheels, etc., have been so great that little reliance has been placed upon these computations. The great bulk of the data has been derived from computations based upon the direct measurement of velocities throughout the cross section of a stream at times when standing at various heights.

At the selected points daily observations of the height of water are made and by means of these and the occasional discharge measurements computations are had of the daily discharge. From the facts thus afforded graphic diagrams have been constructed and averages drawn for the month, the results being expressed in cubic feet per second or in depth in inches of the run-off from the entire drainage basin. The diagrams afford a means of comparing the regimen of one river with another and of noting fluctuations day by day or year by year.

THE DETERMINATION OF THE LAMP HOURS PER DAY AVAILABLE FOR ELECTRIC LIGHTING FROM A STORAGE BATTERY PLANT DRIVEN BY A TWELVE-FOOT AERMOTOR. By Prof. C. L. CRANDALL, Cornell University, Ithaca, N. Y.

IN estimating the cost of operating a small power plant the cost of attendance may form a large percentage of the total. It is this fact, the absence of a fuel account and the low cost of maintenance proper, which allows the windmill still to compete successfully with other prime movers for small units.

The uncertainty in the supply is not very serious for pumping, where storage can be provided, and for many other purposes. Its effect how-

ever is to increase the size of mill, and hence the interest and depreciation charges, for a given service. This is especially the case for electric lighting where the cost for storage by a storage battery forms an important part of the total and increases rapidly with the storage capacity.

On this account the following estimate of the daily lamp hours available for lighting with a ¾ K. W. dynamo and a twelve-foot aermotor may be of interest.

The power of the mill for different wind velocities is taken from tests of a similar mill, published by E. C. Murphy in the Engineering News, Aug. 19, 1897. The efficiency of the dynamo, and the current of given voltage required per lamp, are obtained from the tests at the physical laboratory of Cornell University. The efficiency of the storage battery is estimated. The wind velocity is taken from the records of the New York State Weather Bureau at Ithaca, for a year beginning July 1, 1897.

From the power of the mill and the efficiency of the dynamo and storage battery, the curve of ampere hours at the lamps for a fixed voltage is drawn with wind velocity as abscissa.

With this curve and the wind record, the ampere hours per day, at the lamp, are taken out and plotted as ordinates with time as abscissa. With low winds, the curve approaches a horizontal direction indicating a minimum supply. Tangents are drawn at these points and extended to the left at angles corresponding to different lamp hours per day. The greatest ordinate upward from a tangent to the curve will give the storage capacity required to meet the assumed daily consumption.

ENERGY RECEIVED FROM THE SUN. By Prof. L. G. CARPENTER, Fort Collins, Colo.

IT is well known that the sunshine on our Western plains is much greater in amount than that received by the East. For nearly ten years the author has maintained observations on the intensity of the energy received from the sun (using the Arago-Davy conjugate thermometers) and also observations on the amount of sunshine. The observations show an intensity of from seven to seventeen calories per square meter from 8 A. M. to 5 P. M. on sunny days, or from one-half to one and four-tenths horse power per square yard. It seems as reliable as the wind which is extensively used as a source of power for small plants.

EOLOTROPY IN STEEL. By Prof. THOMAS GRAY, Terre Haute, Ind.

THIS note referred to a peculiarity in the elastic properties of a specimen of flat bar steel which was exhibited at the meeting. The bar was 0.41 inches wide by 0.063 inches thick. It had been heated in the smithy

fire and allowed to cool slowly in the air. When the bar was held like a flat spring and bent first in one direction and then in the reverse direction, the elastic limit was found to be fully three times as high for bending in one direction than for bending in the other. The values of the bending moment at the yield point were found to be 8.9 and 30.8 inch pounds respectively.

It seems that the heating in contact with the hot coal in the smithy fire had produced properties on one side of the bar approximating to those possessed by cast iron, namely, greater elastic strength against compression than against extension.

COMPARISON OF THE RHEOSTAT AND THE SERIES PARALLEL CONTROLLER FOR STREET ELECTRIC CAR SERVICE. By Prof. THOMAS GRAY, Terre Haute, Ind.

IN the spring of 1896 a number of experiments were made at Louisville, Ky., by members of the senior class of the Rose Polytechnic Institute on the relative efficiency of the above two methods of controlling the current flowing through the motors of an electric car. In the rheostat method the motors were in parallel and the total current passed through a rheostat having a maximum resistance of 4.2 ohms. In the series parallel method the rheostat was first used with the motors in series and after the resistance had been cut out the next step of the controller lever threw the motors in parallel and at the same time put each in series with half the rheostat. After this the resistance was cut out and the two motors left in parallel. The final conditions were then the same in both cases. The tests included the measurement by means of an integrating watt meter of the watt hours required to make a round trip of 5.5 miles the car being stopped forty times during the trip An autographic record was taken of the variation of activity during the start. The rheostat method gives maximum activity at the initial contact after which there is gradual diminution as the car attains speed combined with sudden increases as the successive coils of the rheostat are cut out. The series parallel method gives the maximum activity when the motors are first put in parallel and thus after the car has attained a speed of four or five miles per hour. The consequence is that more of the activity is used for propelling the car and less in overcoming resistance. There resulted a comparative economy of from 10 to 15 per cent under the conditions of the test. The ratio of the works done in starting the car in the two cases as indicated by the autographic record was approximately four-fifths. The total weight of the car used was about 15,300 pounds, the watt hour per car mile averaged about 900 and the maximum speed between stops about 12 miles per hour.

These curves giving the data complete are being published by the Railroad Gazette.

THE THEORY OF HALF-TONE PRINTING. By ROMYN HITCHCOCK, New York, N. Y.

THE printing press has developed along the lines of practical experience into a machine of wonderful excellence, especially as to rigidity and strength It is a cardinal principle with printers that a heavy pressure is necessary to give clear and black impressions on paper and for this purpose the press must be tremendously strong; also that in the printing of engravings, half-tones, etc., the pressure must be so distributed by means of the so-called make-ready process that the black parts are printed with a crushing force while the lighter parts are but gently touched.

The speaker declared that all this was wrong. He contended that the effect of make-ready was really to produce contact between the opposing surfaces in the press and that the heavy pressure applied is unnecessary, if perfect contact can be secured without it. He showed that, by the use of a process invented by Mr. M. L. Severy of Boston, the paper was made to conform to the printing surface with very light pressure and that all the effects of printing could be obtained with the Severy fabric without the excessive force usually considered necessary.

The principle of this new process is uniform distribution of pressure — equal pressure per unit of printing area. Specimen prints of half-tones were shown in support of the contentions of the paper, printed without any make-ready. The new theory of printing is thus compared with the views generally held by practical printers.

The old theory requires "unequal distribution of pressure;" the other, uniformity. One demands heavy pressure for blacks; the other light pressure throughout. One makes contact between two hard, rigid opposing surfaces; the other substitutes for one of these a flexible, yielding backing, by which the paper is made to conform to the inequalities of the printing surface. It has been shown that the object of make-ready is not to adjust pressure, as usually supposed, but to insure perfect contact and conformity between surfaces. It is important to recognize this fact. Once understood, the truth becomes evident, that the maximum pressure required in printing is only sufficient to cause a transfer of ink to paper. Hitherto great power has been required to bring these surfaces together and it has been erroneously supposed that this was necessary to cause the transfer of ink.

THE DEVELOPMENT OF THE TOPOGRAPHIC WORK OF THE UNITED STATES GEOLOGICAL SURVEY AND ITS APPLICATION TO THE SOLUTION OF ECONOMIC AND ENGINEERING PROBLEMS. By CHARLES D. WALCOTT, Washington, D. C.

ON THE EFFICIENCY OF REFRIGERATING PLANTS. By Prof. STORM BULL, University of Wisconsin, Madison, Wis.

ON THE MEASUREMENT OF TRAIN RESISTANCE BY DYNAMOMETER. By Prof. F. C. WAGNER, Terre Haute, Ind.

[This paper was published in the Railroad Gazette.]

ON THE USE OF PLATINUM RESISTANCE AS A PYROMETER IN BOILER TESTS. By Prof. F. C. WAGNER, Terre Haute, Ind.

NOTES ON SOME MECHANICAL UNITS. By WM. KENT, New York, N. Y.

IN lieu of Vice-President's address, Professor R. H. Richards of the Mass. Institute of Technology, Boston, presented a paper on Recent Investigations upon The Mechanical Preparation of Ores,—illustrated with lantern slides.

SECTION E.

GEOLOGY AND GEOGRAPHY.

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ADDRESS

BY

HERMAN L. FAIRCHILD,

VICE PRESIDENT, AND CHAIRMAN OF SECTION E.

GLACIAL GEOLOGY IN AMERICA.

INTRODUCTION.

THE life of this Association, with that of its predecessor, covers precisely the period since the glacial theory was introduced to American geologists. It seems highly appropriate, upon the occasion of this jubilee meeting of the Society, to review briefly the history and growth of glacial geology in America and to give credit to the men who were pioneers or who have been most influential in the development of this young and vigorous branch of earth-study.

In the limits of this paper it is impossible to call the roll of all the investigators in American glaciology, and apology is here offered for many omissions and possible errors.[1]

THE GLACIAL THEORY.

Announcement.—The "drift" of the northern United States and Canada was not neglected by the self-taught geologists of the early part of this century. Frequent references occur in the writings of those years to the striæ and polished rocks, the stony

[1] The writer hopes to insert in the record of Section E for the next meeting (1899) a notice of any important additions or corrections which are brought to his attention.

clay, the deposits of sand and gravel and particularly to the conspicuous erratics and perched boulders. These phenomena were commonly attributed to the agency of violent floods, and were called "diluvial." They were, however, so unlike any products of observable aqueous action that the conditions of their formation were a subject of imagination rather than of scientific inquiry. But this conception of water as the chief or only agent concerned in the production of the drift and striæ gained such general assent and so bound the minds of the older American geologists that the glacial theory was not given at first even a respectful hearing, and it was not accepted until long after a majority of foreign geologists had adopted it for western Europe and the British Islands,—not, indeed, until a new generation of workers had possession of the field. The story of the introduction, discussion and final acceptance of the glacial theory in America is an entertaining and instructive chapter in the history of science.

The reports of the early state Geological Surveys, the transactions of learned societies and the volumes of Silliman's Journal down to about 1850 contain frequent references to "diluvial drift," "diluvial scratches," "tremendous currents of water" and terms of similar import. The first suggestion of ice as a contributory agent in the genesis of the drift, in the form of icebergs or ice-floes, was made by Peter Dobson of Connecticut, in a letter to Silliman,[1] dated November 21, 1825. Murchison gives to this letter of Dobson the credit of suggesting to him the iceberg hypothesis, while Edward Hitchcock obtained his similar idea from Sir James Hall of Scotland.

So far as the writer has discovered in print no American geologist suggested land or glacier ice as a possible cause of the drift phenomena until 1837, when T. A. Conrad asserted his belief in such agency in the following words:

"Whence then this immense body of ice which has scattered boulders over so vast a tract of country, appearing too at an epoch subsequent to the extinction of the Mastodon and other mammalia, which evidently lived in this region and enjoyed equatorial climate anterior to the icy

[1] It is not desirable to encumber these pages with a great number of bibliographic references. These can be found in chronologic order under authors' names in United States Geological Survey Bulletin No. 127—"Catalogue and Index of Contributions to North American Geology, 1732-1891," by N. H. Darton. (See "Glaciology," page 417 and "Pleistocene," page 756.) Bulletins Nos. 130 and 135 contain the bibliography from 1891 to the close of 1895.

period? Nothing can reconcile this apparent contradiction but the admission of a fall of temperature far below that which prevails in our day, freezing the enormous lakes of that period and converting them into immense glaciers, which probably continued undiminished during a long series of years."

We cannot, however, regard Conrad's suggestion as a fair presentation of the glacial theory. It was made only incidentally, as an argument for his hypothesis of epochs of cold climate, and without any attempt to give a full statement or to marshal the facts as bearing upon the genesis of the drift. It is significant, however, as indicating, by the casual way in which he introduces the argument, that the conception of great bodies of land ice might not have been, even at that time, an entirely new idea to American geologists. The work which then had been prosecuted for some years upon the alpine glaciers was probably not entirely unnoticed by Americans.

The American geologist who first gave a favorable reception to the glacial theory of Agassiz, as far as printed records show, was Edward Hitchcock. In his address as retiring President at the second annual meeting of the Association of American Geologists and Naturalists, held at Philadelphia, April, 1841, he described the characters of the drift and the various phenomena which we now call glacial with great accuracy and appreciation. After discussing several hypotheses of the drift he writes as follows:

"But the recent work of Agassiz, entitled 'Etudes sur les Glaciers,' gives a new aspect to the subject. . . . . Henceforth, however, glacial action must form an important chapter in geology. While reading this work and the abstracts of some papers by Agassiz, Buckland and Lyell, on the evidence of ancient glaciers in Scotland and England, I seemed to be acquiring *a new geological sense*; and I now look upon our smoothed and striated rocks, our accumulations of gravel, and the *tout ensemble* of diluvial phenomena with new eyes."

" . . . . . I do not mention these difficulties (to which I might add more) as any strong evidence against this theory. For so remarkably does it solve most of the phenomena of diluvial action, that I am constrained to believe its fundamental principles to be founded in truth."

In the application of the hypothesis to American drift he stated his chief difficulties to be: (1) the general southerly direction of the striæ and drift transportation over New England and New York, instead of radiating from the highlands as in the Alps; (2) the lifting of the erratics from lower to higher levels, although

for this work he regarded ice as more efficient than water; and (3), the localized drift masses which we now call "drumlins." At that time, with only Alpine glacier phenomena as the foundation of the theory, these were valid and forcible objections. Indeed his presentation of the argument for and against glacial agency showed excellent appreciation and grasp of the subject, and to-day can be read with profit. This is also true of his "Postscript," discussing the glacier hypothesis and moraines in the introduction to his Geology of Massachusetts, 1841.

In these writings Professor Hitchcock seemed to declare his adherence to the glacial hypothesis, as applied to America, with as little hesitation and qualification as would be expected of a careful man of science in espousing a new theory that antagonized the prevailing belief and prejudice of his fellow workers. It is evident from subsequent records that his utterances were accepted at the time as committing him to the acceptance of the theory of Agassiz. But, unfortunately for truth and for American geology, the circumstances and scientific forces of that time did not allow him to stand upon the advanced ground he had taken. He was criticised by Murchison, to whom he was attached, and evidently found no support among his confrères at home. Instead of following Buckland in support of the glacial theory he went over to the side of Murchison and Lyell, and the next year at the Boston meeting his elaborate paper on the drift favored the iceberg hypothesis. In 1843 he is quoted in the following words:

"Professor Hitchcock remarked that so disastrous had been his experience in respect to the glacial theory of Agassiz that he was almost afraid to say anything more on the subject. . . . . He had been supposed to be an advocate for the unmodified glacial theory. But if he could trust his own consciousness he had never been a believer in it. . . . . By this term (glacio-aqueous) he meant to say that the phenomena of drift were the result of joint action of ice and water, without saying which of these agents had exerted the greatest influence. But whether that glacio-aqueous action had been the result of the enormous accumulation of glaciers, according to Agassiz, or from floating icebergs . . . . . according to Lyell and Murchison, or of the upheaval of the Arctic ocean whereby its aqueo-glacial contents were precipitated southward according to De la Beche, he had not then made up his mind nor has he yet made it up."

Professor Hitchcock's attempt to find some compromise between the glacial and iceberg theories of the drift was a failure. He came very near to being a glacialist. If he had followed his own

perception of the truth he would have been the leader in a great forward movement in geological science, which, by his defection at the critical moment, was suppressed for twenty years.

Discussion. — The printed accounts of the annual meetings of the Association of American Geologists and Naturalists give evidence of a lively interest in the subject of the drift, but there seems to have been no champion of the discredited glacial hypothesis; or if so he was not thought worthy of record. There was much difference of opinion as to the agencies of the drift but all were hostile to the new theory. The brothers Rogers, with Emmons, Jackson, Nicollet, Redfield, Couthuoy and others, lost no opportunity of upholding some form of the diluvial hypothesis and for ten years no American geologist ventured openly to adopt and proclaim the theory of Agassiz. Doubtless there were various reasons for this unreceptive attitude, some of which we can discover. Lyell came to America in 1841, and in 1842 he attended the third meeting of the Geologists, held at Boston, and brought the reinforcement of his great reputation to those who favored the hypothesis of submergence and icebergs, of which he and Murchison were the great apostles. The supporters of this theory probably did not clearly understand the genesis of icebergs and did not realize that their hypothesis simply pushed the glacier area some distance northward. The scientific objections to the glacial origin of the drift were to them conclusive. They were unable to apply to the broad plateaus of northeastern America the work of local or alpine glaciers. How could ice flow up hill? The glaciers of the Alps all moved down the slopes. How could an ice mass move over even comparatively level tracts? Indeed, Dana did not answer this difficulty until the 1879 edition of his Manual. The prevailing southerly direction of the drift movement, with only minor deflections by mountain masses or great valleys, did not harmonize with conceptions derived from written descriptions of the glaciers of the Alps, and they were not prepared to grasp Agassiz's idea of a great circumpolar ice sheet. The very large proportion of water-worn or water-laid materials in the drift certainly favored aqueous agency.

But there were other antagonistic forces which were not of a scientific character. Agassiz was a comparatively young man and quite unknown in geology except for his studies of glaciers. How could his opinion weigh against those of the giants in geology? Another power, which will scarcely appear in the scientific writings

of the time, but which was a great conservative force, was theological opinion. All hypotheses invoking water as the drift agency might be harmonized with belief in the Noachian deluge, but the Bible gave no countenance to an ice deluge. The "diluvium" was an ever present evidence of the truth of the deluge. To explain it away was little better than heresy. In these days of liberty of thought it is not possible for us to realize the repressive power of religious opinion in Puritan New England. Furthermore the diluvial hypotheses were unduly deductive, and like all opinions not based on observational or inductive evidence did not readily yield to the arguments derived from facts. The older geologists had made up their minds and that settled it. They were also mostly committed in print to some form of the diluvial idea. Those early classics in American geology, the state reports of New York, Massachusetts and New Hampshire, appeared during the years 1841–1844, and by generally referring the drift to aqueous origin helped to fix opinion along diluvial lines. Before leaving this subject it will be well to state briefly the nature of some of the theories of the drift.

Edward Hitchcock had written the Geology of Massachusetts before he became fully acquainted with the theory of Agassiz. He presents the several hypotheses for the drift, but rules out glaciers, retains the term "diluvium," and favors the idea of waves of translation from the arctic region. He displays, however, the openness of mind and keen perception which led him virtually to adopt the glacial theory upon its first fair presentation, by distinctly saying that no theory proposed at that time was satisfactory to him. His "Postscript," describing glacial phenomena, at the beginning of the volume was an after-thought, dated 1841.

Lardner Vanuxem in his Geology of the Third New York District, 1842, referred briefly to the drift, compared the iceberg and glacial theories and gave preference to the latter in some modified form, suggesting the term "local ice" for "glaciers."

In the same year Ebenezer Emmons published the Geology of the Second New York District. He regarded the iceberg theory as fully established. The rock scorings and finer drift he thought were produced by broad shallow currents or rivers, at an earlier epoch, while during a later marine submergence the floating ice had transported the erratics and coarser drift. Glaciers were entirely ruled out.

W. W. Mather in the Geology of the First New York District,

1843, gave an exhaustive account of the drift. Regarding the glacial theory he had the objections noted by Hitchcock. He favored currents of water and floating ice, and thought that the greater currents were due to a relatively sudden collapse of the earth's crust, producing accelerated velocity of rotation, and hence quickened oceanic currents.

In the Geology of the Fourth New York District, 1843, James Hall held views similar to Emmons. He had not seen the writings of Agassiz, and probably very few American geologists had at that time, but he could not admit glacial agency as there was no sufficiently elevated land-mass upon the north.

C. T. Jackson, in his Geology of New Hampshire, 1844, quoted M. de Luc against the glacial theory, even as applied to Europe, and regarded its application to America as absurd. He attributed the drift to Arctic waters and ice, they "having been hurled with great violence over the surface of the northern hemisphere."

In a review of Dr. Jackson's report, T. T. Bouvé wrote as follows:

"In view of all that is known of the movement of diluvium in this country we cannot but regard the glacial theory as wholly inedequate to produce the results met with, and we think with Dr. Jackson that the grooves on the rocks, if produced by glaciers, should radiate from our principal mountain ranges and should be more abundant in their immediate vicinity, while they should be wanting in the level country and over our extended tablelands!"

The most ardent and extreme champion of De la Beche's hypothesis of catastrophic waves or debacles of water was Prof. H. D. Rogers. The following quotation from his Presidential address before the fifth meeting of the Association of American Geologists, held at Washington, May, 1844, will give a sufficient idea of this hypothesis. After discussing the drift at great length and reviewing all the explanation that had been proposed, he gives the following as his own views:

"If we will conceive, then, a wide expanse of waters, less perhaps than one thousand feet in depth, dislodged from high northern or circumpolar basin, by a general lifting of that region of perhaps a hundred feet, and an equal subsidence of the country south, and imagine this whole mass converted by earthquake pulsations of the breadth which such undulations have, into a series of stupendous and rapid-moving waves of translation, helped on by the still more rapid flexures of the floor over which they

move, and then advert to the shattering power of the tremendous jar of the earthquake, we shall have an agent adequate in every way to produce the results we see, to float the northern ice from its moorings, to rip off, assisted with its aid, the outcrops of the hardest strata, to grind up and strew wide their fragments, to scour down the whole rocky floor, and, gathering energy with resistance, to sweep up the slopes and over the highest mountains."

To us at the present time it would seem as if the mere statement of this hypothesis should have been sufficient for its own refutation. Certainly it was so far-fetched, calling in an agency entirely outside the range of human observation and knowledge, and at the same time denying the competency of an observable agency which was at that time reproducing some of the most important drift phenomena, that it seems unscientific to the last degree. Yet, in 1849, three years after Agassiz had come to America and long after the detailed arguments for glacial agency had been fully and clearly set before American readers, Professor Rogers wrote:

"V. The Diluvial Hypothesis, as modified by myself, assumes, not the submersion of the land but a series of violent elevatory earthquake movements, in the arctic regions, displacing the arctic waters, with the ice which bound them, and sending these southward, across the northern districts of the continents."

As early as 1842 the facts concerning the geologic work of the Alpine glaciers and the arguments for the glacial theory had been placed before American readers by the republication in Silliman's Journal of an excellent article by Charles Maclaren. And in the next year an elaborate review in the same Journal, of James Forbes' noble book on glaciers left American geologists with no excuse for lack of general knowledge of the glacial theory and its reasonable claims.

Late in 1846 Louis Agassiz arrived in America. In 1847 he explored the White Mountains and found evidence of local glaciers. In 1848 he visited the great lakes and found abundant evidence of continental glaciation. In September of that year he attended the first meeting of this Society, at Philadelphia, and described the glacial phenomena about Lake Superior, showing the identity of the phenomena in America with those in Europe. To his personal testimony was added that of Arnold Guyot and Edmund Desor, who had been colaborers with him and had also brought their ample experience to bear upon the American field. The reception by

the older geologists of this personal and expert evidence was evidently not of such a character as to encourage Agassiz in further presentation of the matter. In his book on Lake Superior, published 1850, he wrote in a somewhat sarcastic vein as follows:

"But such is the prejudice of many geologists that those keen faculties of distinction and generalization, that power of superior perception and discrimination, which have led them to make such brilliant discoveries in geology in general, seem to abandon them at once as soon as they look at the erratics."

Agassiz did not present another glacial paper before the Association until 1870, his chief work in the meantime lying in the domain of zoölogy. However, from about 1850 the extreme diluvial notions were either abandoned or presented less aggressively.

At the second meeting of this Association, in Cambridge, 1849, there was the usual animated discussion of the drift, following papers by H. D. Rogers and Arnold Guyot. An extended paper by the latter described the glacial phenomena in the Alps, and stated that those of the White Mountains were similar. The diluvialists still opposed the glacier theory, even to the degree of questioning the Alpine observations of Guyot and Agassiz. James Hall evidently voiced the sentiments of the younger men when he said, as reported in the discussion:

"In this country there had been a tendency to oppose the glacial theory because it had been applied for the explanation of diluvial phenomena. We ought to be ashamed in this country, he said, to speak with confidence, after so little research, when there had been so much abroad."

From this time there are fewer references to the origin of the drift. A new generation of geologists was taking the place of the older men and they were freer to accept the new ideas while not aggressively attacking the prevailing ones. The writings of the younger men frequently refer to glacial agency as an admitted fact. Among the contributions to glacial geology in those earliest years of the science but very few can be noticed. In 1850 C. B. Adams, at the New Haven meeting of the Association, read a paper on changes of level in North America, in which, adopting the hypothesis of Lyell, he suggested the elevation of northern land as sufficient cause of the ice sheet; also of submergence at the close of the glacial period to account for the undisputed marine deposits. This hypothesis was elaborated by Dana in his Presi-

dential address of 1855, and placed upon a working basis by three time divisions: (1) Glacial epoch, with northern elevation of land; (2) Laurentian epoch, of depression; and (3) Terrace epoch, of moderate reëlevation to present altitude. From this time Dana was the leader of the glacial forces and the chief exponent of the theory.

In 1857 Edward Hitchcock published in the Smithsonian Contributions his extended and valuable paper on Surface Geology, treating chiefly of the Connecticut valley. He distinguished local glacial striæ from the general drift striæ, and recognized moraines and other effects of local glaciers. In 1859 C. H. Hitchcock made the same discrimination for New England in general. The Geology of Vermont, published in 1861, while describing phenomena of local glaciers, attributed the common drift to icebergs. In 1863 the report of progress of the Geological Survey of Canada from its beginning to that date, contained a chapter on surface geology, written by Robert Bell, which attributed the drift phenomena to ice and fully accepted the glacial theory, even recognizing moraines in the Ottawa valley.

Acceptance.—It will be seen that the acceptance, in America, of the glacial theory was by slow degrees. It involved the displacement of preconceived deductive hypotheses, and was opposed by the modified and not unreasonable theory of submergence and floating ice, held by Murchison and Lyell, which still has some qualified support. During the decade from 1850 to 1860 there was comparatively less discussion of the drift problem. Geologists were readjusting their ideas and taking new bearings. Then followed the years of the war of the Rebellion, when the thought of Americans was turned away from abstract science and even the meetings of this Society were suspended. However, in 1862, the glacial theory received two notable reinforcements. One was, in England, its public adoption by Ramsay and his abandonment of the iceberg theory; the other, in America, the publication of the first edition of Dana's Manual of Geology. In this epoch-making book in geological science the whole problem of the drift was clearly presented. The arguments for and against both the iceberg and glacial theories were stated with a logical conclusion in favor of the latter, allowing the work of icebergs along the continental borders and across deep channels. An unbiased reader could judge for himself of the merits of the two theories but could scarcely fail to

accept the conclusion of the author. The glacial epoch as a division of Post-Tertiary time was given its place in the geological time series; and the various elements of the glacial problem were placed before student and worker so as to be available as a basis for investigation. The foundation was laid for the construction of glacial geology, but the time was not propitious for its completion.

Following the close of the war, interest was renewed in glaciology, and two papers by Charles Whittlesey, in 1866 and 1867, are worthy of note. In the latter year a masterly paper by Edward Hungerford, before this Association, discussed the physical conditions and climate of the glacial period. He did not agree with Agassiz's extreme views of great southern extension of a polar ice-cap; and his moderate conclusions are, in general, accepted to-day.

At this opportune time, 1867, when the greatly stimulated thought of the people was turned into peaceful ways, the revised edition of Dana's Manual carried the glacial theory into every college of the land as the evolutionary product of the discussion upon the drift during the fourth of a century. However, the theory still found some opposition, and as late as 1871 Dana said that American geologists were divided on the iceberg and glacial theories, and he deemed it necessary to give further proofs against the former.

When shall we say that glacial geology had its beginning in America? The discussion of the theory began in 1841 with the address of Edward Hitchcock. The acceptance of the theory by any number of Americans could not have been earlier than the personal advocacy by Agassiz and Guyot in 1848 to 1850. In some qualified form the theory was doubtless adopted earlier by a few independent thinkers, as Conrad and Vanuxem. The general acceptance of the theory and the making of it a basis for systematic work and investigation were probably not before 1867.

The periods in the history of drift-study in America may be stated as follows:

Undisputed reign of diluvial hypothesis—to 1841.

Discussion of the glacial hypothesis—1841 to 1848.

Gradual adoption of the glacial theory—1849 to 1866.

Development of glacial geology—1867 to date.

THE ICE BODY.

Centers of Dispersion.—The early views of the unity and simplicity of the ice-cap, following Agassiz's extravagant hypothesis of hemispheres of ice, did not long endure the test of advancing knowledge. The direction of striæ and the trend of peripheral moraines were soon recognized as indicating complexity of growth and of movement. In 1871 Dana argued for multiple centers of radial movement, and he referred the New England glacier to a source between the St. Lawrence and Hudson Bay. In 1879 he said there had been no ice-cap enveloping the whole polar region, and recognized, also, that an inclination of the land surface was not necessary for the movement of the ice-body. In 1880 W J McGee expressed the view that the polar regions were never more extensively glaciated than at the present time. Similar views were expressed by H. Carvill Lewis in 1886, who designated two centers of ice accumulation and radiation, on the east and west sides of Hudson Bay, and in 1889 by Robert Bell, the Nestor of Canadian glacialists.

Dr. G. M. Dawson in 1888 recognized a separate confluent ice mass in British Columbia between the Coast and Rocky Mountain ranges, and in 1890 he gave names to two ice bodies, Cordilleran for the one in British Columbia, and Laurentian for the eastern one, which he regarded as surrounding Hudson Bay. In 1895 J. B. Tyrrell restricted the name Laurentian (Laurentide) to the mass east of Hudson Bay, and gave the name Keewatin to the body west of Hudson Bay. These three centers ("glacial radiants" of Claypole) were adopted by Professor Chamberlin in his chapter on North American glaciology in the latest edition of Geikie's "The Great Ice Age." Dr. Dawson thinks the Keewatin and the Laurentian (Labradorian) centers should be regarded as constituting the "Laurentide group" as distinguished from the far-separated Cordilleran mass.

Migration of Maxima.—The first definite suggestion of lack of contemporaneity in the growth and culmination of the glacial centers was made by Robert Chalmers in 1890. From study of till sheets, boulder trains, etc., in the area of contention between adjacent ice centers Dr. Dawson, in 1895, thought there had been a migration of glaciation from the Cordilleran to the Laurentian

plateaus; and in 1896 Mr. Tyrrell made a further refinement by claiming that the Keewatin glacier center had its maximum later than the Cordilleran and earlier than the Labradorian; and that a fourth center over Greenland is in existence to-day. This theory of migration of maximum accumulation receives encouragement from the results of recent studies of the Alaskan and the Greenland ice sheets. While the Alaskan glaciers are believed to have been waning in later time, the Greenland ice cap has probably increased, at least on the east coast, during several centuries, or since the time of the Norse colonies.

Thickness.—In 1883 J. C. Smock published an article on the thickness of the ice over New England and the Middle States. Later observations have shown that the thickness of ice was sufficient to bury the highest mountain peaks within the ice field. Explorations of the Greenland ice cap have yielded data concerning the curving slope of the margin of the ice sheet which seem in the hands of Chamberlin and Upham to give a reliable basis for the comparison with ancient ice sheets. Mr. McGee estimated the thickness in northeastern Iowa as "500 feet a few miles from the margin, to an indefinite but not very much greater thickness in the interior of the ice body."

Direction of Flow.— Previous to 1879 W J McGee was able, by the study of topographic forms of the drift in northeastern Iowa, to determine the direction of ice flow without evidence from striæ. It is now recognized that drumlins and eskers are more trustworthy criteria of general direction of ice movement than the glacial striæ, which are more subject to the effect of local causes. Peripheral moraines are demonstration of the direction of flow at the margin of the ice sheet. By these several criteria it is now demonstrated that the ice movement followed the trend of the larger depressions and valleys. As early as 1863 Dana argued, from direction of striæ, for distinct glacial flow in the Connecticut and Hudson valleys. The very decided localization of movement in the basins of the Laurentian lakes was shown cartographically by Chamberlin in 1877.

Lobing of Margin.—This is closely related to the flow in the longitudinal valleys, and is produced by the concentration or massing of the marginal ice in such depressions. This was also shown by Chamberlin in 1877 in his map of terminal moraines; those of the ice lobes filling the basins of the great lakes having a

conspicuously looped, festooned or crescentic character. Further study has emphasized the lobate form of the periphery of the ice sheet, at least during the recessional phase.

Driftless Areas.— The surprising and apparently inexplicable phenomenon of an area unaffected by ice action but wholly surrounded by glaciated territory was brought to notice by J. D. Whitney in 1862. Much doubt was felt regarding the nature and even the existence of this driftless area until the observations were fully confirmed by the detailed descriptions and illustrations in the elaborated paper by Chamberlin and Salisbury, published in 1886. The explanation is supposedly found to lie in the peculiar relation of ice movement through the basins, the ice being diverted from this area. Until recently this area was thought to be unique but in 1891 Professor Salisbury discovered another small driftless area in western Illinois.

Transportation of Drift.— The precise manner in which the ice carried its burden of rock debris has been in question. Mr. Upham has emphasized the amount of englacial drift. He argues that the differential flow of the ice induces ascending currents which lift the basal or subglacial drift into higher planes of the ice body. On the other hand Professor Chamberlin claims that the continental ice mass never held any considerable burden of englacial drift except near the base; and he finds support for his position in the essentially basal character of the debris in the Greenland glaciers. The existence of ascending currents in the ordinary flow of the ice sheet is not admitted by most students of glacial physics. The question is of some moment in relation to the manner of accumulation of localized masses of debris, such as drumlins, moraines and eskers.

As a philosophical discussion of the character and work of a local ice-body, the writings of W J McGee upon the glacial phenomena of northeastern Iowa deserve special mention.

GLACIAL PERIOD.

Cause. — The cause of the glacial period remains quite as much a mystery as it was in 1840. A large body of fact has been collected but it points in different directions. Every person has entire liberty of opinion; most glacialists have no opinion at all upon this subject. Up to 1875 the Lyellian hypothesis of land elevation

as the cause of the cold climate and snow accumulation had the majority of adherents. In America this was proposed by C. B. Adams in 1850, and was adopted by Dana in his Presidential address of 1855 and was held by him during his life. It has been the consistent teaching of the Manual of Geology. Of preglacial elevation of northern land there is little doubt, but many glacialists find reasons for thinking that during the latter part of the glacial period the land was lower than at present, and certainly was so at the close.

Croll's "Climate and Time" presented a plausible astronomical hypothesis, the concurrence of variable elements in the relation of the earth to the sun affecting alternately the north and south polar regions. For a time this won large assent; but objections soon multiplied. The secular periods of glaciation apparently required by this hypothesis in preceding geologic time are quite lacking, although glacial drift deposits occur as far back as the Permian. The most serious objection is the absence of glaciation over vast areas of arctic lands, as Alaska and Siberia. Even the physical competency of the astronomic changes has been questioned, and the hypothesis has fallen into neglect as the tendency of late is to concentrate attention on phenomena and more immediate inferences.

A change in the axis of the earth is a suggestion that finds few supporters. An hypothesis of atmospheric change, a variation in the amount of the carbon-dioxide content of the atmosphere, has recently received the support of Professor Chamberlin.

The opinion of the majority of geologists may probably be fairly stated by saying that the various elements affecting climate, geographic, atmospheric and astronomic, are thought to be so nicely balanced that a comparatively slight change or maladjustment may produce serious climatic effects.

Time Divisions. — Professor Dana in 1855 proposed the tripartite division of the glacial period, correlating with land oscillation, into (1) the Glacial epoch with high elevation of land; (2) the Laurentian (Champlain), with depression of the land; and (3) a transition epoch, called the Terrace, during which the land rose to its present level and the river terraces were cut by the enlivened streams. This scheme was elaborated in the first edition of his Manual, in 1862, and adhered to with slight changes in all subsequent editions. In the 1879 edition he gave the Ter-

race epoch a formal place, changing the name in the last edition to "Recent." The name "Champlain," originally given to the marine clays by Edward Hitchcock, was substituted for "Laurentian" in the edition of 1867.

While this terminology has been generally used and the time divisions adopted in a general way, some modifications have been thought desirable. It is believed by many that the Champlain subsidence was far along, perhaps at its maximum, before the ice sheet retreated. Indeed, Mr. Upham would make the Champlain only the closing part of the Glacial epoch. The "Recent" epoch is now regarded as essentially a part of the present epoch, and this view is strengthened by the demonstration, by Mr. Gilbert, that the northward differential uplift of at least the Laurentian basin is still in progress.

Interglacial Epochs. — Dana held firmly to the essential unity of the Glacial epoch, and in this he was largely followed, especially by the men who were trained upon the comparatively simple, homogeneous drift sheet of the Eastern States. But in the broad areas of the west the drift is not so simple. Early in the study of the region buried soils, peat beds and other evidences of deglaciation were found intercalated in the drift, and were noted by Whittlesey, Newberry, Orton, N. H. Winchell and others. In 1873 Newberry regarded the "Forest Bed" as marking a distinct deglacial condition in the drift period. In 1878 McGee described vegetal deposits in the drift of northeastern Iowa of such depth and areal extent as to indicate an interglacial epoch, and he discriminated a later from an earlier till. Subsequently Chamberlin accepted these conclusions, and in 1882 he discovered the greater southern extension of the older drift and noted the evidences of a long interval between. This idea of the duality or multiplicity of the glacial epochs met with long-continued opposition, but was found by the glacialists of the west a good working hypothesis, and evidences in its favor have multiplied.

In 1889 Chamberlin regarded the fringe in Pennsylvania as the unburied edge of the older drift, and in 1891 Salisbury described the extra-morainic drift of New Jersey and Pennsylvania as the weathered and eroded border of a drift sheet far older than the moraine-bordered till. This was the first recognition in the Eastern States of multiple drift phenomena, comparable to that in the Western States.

The study was given definiteness and placed on a working basis by the admirable article of Professor Salisbury, in 1893, on the "Criteria for recognition of distinct glacial epochs." The application of these criteria by different workers, particularly Leverett, Coleman and Hershey, has so strongly confirmed the view of distinct ice invasions separated by intervals of deglaciation that this has passed quite beyond the point of argumentation, and the question now is as to the extent and duration of the stages of deglaciation, or the relative length and importance of the glacial and interglacial time divisions.

In 1894, Professor Chamberlin presented the whole matter, in condensed form, in Geikie's "The Great Ice Age," and set the example, subsequently followed by Geikie, of denoting the several glacial time divisions by geographic names, using the names of areas where the individual deposits are typical. In 1895 he gave similar names to the interglacial epochs. The contemporaneity in Europe and America of the climatic oscillations and of the epeirogenic movements, at least in a general way, seems quite demonstrated by the studies of Chamberlin and Geikie.

Time subsequent.— A question of great popular interest concerning glaciation is that of time, — the time in years, especially since the ice disappeared. Glacialists will in honesty have to admit that they cannot yet fully satisfy that proper curiosity. That the time is very brief, judged by geologic time standards, since the ice sheet finally disappeared from our region, seems certain. Judging from the freshness of the glacial scorings and the deposits, 5,000 or 10,000 or 15,000 years is thought by many glacialists to be a fair estimate of the length of their exposure. But no reliable chronometer has yet been found. Niagara gorge has been regarded as the best timepiece, but fuller study has made the "Niagara Problem" more complex and uncertain. It seems likely that the volume of Niagara's water has varied greatly for long periods, as the history of the river is intimately connected with that of the glacial lakes of the upper lake basins. It is admitted that during the ice retreat the Laurentian basin was lower, as regards sea level, than at present; and was progressively lower northward and northeastward. It also seems certain that at the time of the ice removal the waters found lower outlets eastward from lake Huron than by way of lake Erie, and that such outflow persisted until the slow differential uplift raised the northern outlet above the St. Clair outlet. Hence

for one, or probably two, long stages Niagara river carried only the insignificant drainage of the Erie basin. This argues for greater length of postglacial time. On the other hand there is a question as to the origin of the gorge, whether cut *de novo*, or in part reëxcavated or enlarged by the modern river. The chief students of the subject have been Gilbert, Spencer, Upham and Taylor. Out of the mass of literature two papers must be noticed: that of Gilbert, "The History of Niagara River," 1890, which placed the elements of the problem before the public; and the paper by F. B. Taylor, 1898, "Origin of the Gorge of the Whirlpool Rapids at Niagara," which gives the latest results of the correlation study of Niagara and the glacial lakes.

The recession of St. Anthony's Falls in the Mississippi has also been studied as a measuring rod of postglacial time, especially by N. H. Winchell, with results concordant with those of Niagara and other similar studies.

Recently Mr. H. M. Bannister has argued for great duration of the ice sheet, from data derived from transportation of far-travelled erratics; and Mr. Taylor finds a great length of time required to form the moraines of recession in the Erie-Huron basin laid down by the slow oscillations of the ice front; the latter paper favoring astronomical forces as a cause of glaciation, at least in part.

The question of glacial time is closely connected with the problem of glacial cause.

INTERPRETATION OF SPECIAL PHENOMENA.

Drumlins. — These are usually conspicuous topographic features and frequent references to them, under various descriptive terms, occur in the early literature of the drift. Under the diluvial hypothesis little effort was made to explain their origin, and Edward Hitchcock found in them an objection to the glacial theory.

The name "drumlin" was applied in 1866 to similar drift masses in Ireland by H. M. Close. In America various appellations were given: by Professor Shaler, who was the first American to write upon them, — "drums" and "sow-backs," 1870; by C. H. Hitchcock, "lenticular hills," 1876. The elongated forms of Central New York were described in 1882 by Laurence Johnson as "parallel drift hills," and Professor Chamberlin in 1883 called them "mammillary" or "elliptical hills." The name drum-

lin was proposed for use in this country by W. M. Davis, in 1884, and at once adopted.

As nearly all drumlins are composed wholly of till, and especially so in the superficial part, it was early recognized that they were an immediate product of ice work, and they were so referred by Close as early as 1864. In 1872 Kinahan and Close regarded them as subglacial and constructive, comparing them to the longitudinal sand-bars in the bed of a stream. This view of the genesis of drumlins has been adopted, in a general way, by most American glacialists. The suggestion, made by Professor Shaler in 1870, that they may be remnants of an earlier, eroded drift sheet, or that they represent old moraines remodelled by readvance of ice, as proposed by C. H. Hitchcock, have been quite abandoned. Their parallelism with the direction of ice movement was first noted in America by Hitchcock and Upham in 1875. In 1883 Professor Chamberlin discriminated drumlins from moraines, as being longitudinal, or axi-radiant with reference to the moving ice body.

The hypothesis of drumlin origin by the piling of the ground-moraine by differential pressure and movement of the overriding ice body, along the peripheral zone where transporting power becomes inadequate to the drift burden, is regarded as a plausible and satisfactory explanation. The chief objection to this is negative, the failure of more uniform distribution and even the entire absence of drumlins over vast areas of drift. Another difficulty is physical: a lack of precise knowledge of the manner in which ground moraine is lifted in relatively short distances to heights of 100 or 200 feet. Mr. Upham has attempted to solve this problem by an argument for the construction of drumlins, and moraines also, by the lodgment of englacial drift. This difficulty is less acute, however, since Professor Chamberlin had observed sections of miniature drumlins beneath the ice foot of the Greenland glaciers, with the overriding ice moulding itself to the drumlinoid curve.

Moraines. — The masses of moraine drift were not entirely overlooked in early writings upon the diluvial phenomena, yet they were not emphasized since their superior importance and interest only appear with their recognition as marginal phenomena of the ice sheet. On account of their irregular topography and areal distribution they are not so likely to attract attention as

drumlins or even as some eskers, and not until the glacier theory was so established as to be made the basis of observation and investigation were moraines discriminated and mapped. Their systematic study is within the last twenty-five years.

The earliest mention of moraines, by that name, in American geology is believed to occur in the postscript to Edward Hitchcock's Geology of Massachusetts. (See above, page 262.) Appreciating their significance as glacial phenomena he gives a large space to moraines and notes their occurrence at various localities. The closing paragraph is as follows:

> "Is it possible that the whole of Cape Cod is nothing but a vast terminal moraine, produced by a glacier advancing through Massachusetts Bay and scooping out the materials that now form the Cape? In this case the moraines at Plymouth and Truro would form part of the lateral moraines, and probably most of Nantucket and Martha's Vineyard might be regarded as moraines of the same glacier, when it extended further south."

The hills of the Long Island moraine were described by Mather in 1843, in his report on the First New York District, but without recognizing their true character.

In his chapter on surface geology, in the Report on the Geological Survey of Canada for 1863, Robert Bell described ridges of drift as resembling moraines, along the Ottawa river, which was a correct diagnosis. His map of superficial deposits, accompanying that report, is believed to be the first map of Pleistocene deposits published in America with recognition of glacial agency.

In 1864 J. D. Whitney published a notice of terminal moraines of alpine glaciers in the Sierra Nevada, about Mt. Lyell and Mt. Dana. In 1866 Whittlesey indicated on his map, in the Smithsonian Contributions, "morainic knolls" south of Lake Erie, also west of Lake Michigan, the latter being the noted "kettle range," studied later by Chamberlin, but apparently he did not understand their significance as terminal drift.

In his report of the Iowa Geological Survey, 1870, Dr. C. A. White refers certain ridges, somewhat hesitatingly, to terminal moraines. Excluding the references by Edward Hitchcock this is probably the earliest notice printed in the United States of terminal moraines in the open country, as distinguished from those of alpine glaciers in the mountain valleys.

In 1871 Mr. Gilbert described the series of recessional moraines

in the Maumee valley; and from this time the study of morainal drift was prosecuted with more confidence.

Probably the earliest positive recognition of the great terminal moraine of the continental ice sheet was made by C. H. Hitchcock in a paper on the Long Island moraine read before the New York Lyceum of Natural History in 1868, but never printed. Several years passed before there were any published descriptions of the terminal moraine, and it is interesting to note the comparative suddenness and contemporaneity with which several workers covered the entire line of terminal drift from Cape Cod to the Missouri. In 1875 G. M. Dawson recognized the "Missouri Coteau" as marginal glacial debris. In 1877 T. C. Chamberlin described the Wisconsin kettle moraine and indicated the southern limit of drift from Nebraska to New York. This was the first definite mapping of moraines in American geology. In the same year George H. Cook and John C. Smock published their first paper on the terminal moraine crossing New Jersey, and in 1878 and 1879 Warren Upham published descriptions of the terminal moraine series which he had surveyed from Staten Island to Cape Cod. In 1881 H. Carvill Lewis and G. F. Wright traced the same drift series across Pennsylvania, the results being printed in 1884. Professor Chamberlin's classic paper on the "Terminal Moraine of the Second Glacial Epoch," which has been a basal reference for all subsequent work upon the moraines about the Great Lakes, was published in 1883. It will be seen that within six years, 1877 to 1883, the important terminal and recessional moraines within the limits of the United States were located and described; and although the work in preparation for publication, especially by Professor Chamberlin, must have occupied some years preceding, it is nevertheless a good example of the saltant character of intellectual progress.

In 1878 Professor Chamberlin correlated the Cape Cod–Long Island morainal series with the "Kettle Moraine" belt of the interior region.

A discrimination between the extreme southern tract of the drift, in the Mississippi basin, and the more definite morainal belt of a later ice invasion was made by Professor Chamberlin in 1882. A similar discrimination in Pennsylvania and New Jersey was made by R. D. Salisbury in 1891.

Against the great debt which American glaciology owes to

European investigation there is something on the credit side. The skill acquired in the study of American moraines enabled two American geologists to find similar phenomena in Europe. H. Carvill Lewis, in 1886, traced terminal moraines in Great Britain and Ireland, and one year later R. D. Salisbury did the same thing in Germany, these being the first discovery of open-country moraines of the massive order of the European ice sheets. These discoveries of European peripheral moraines were of great importance, as they laid the foundation for further location of morainal belts and so made possible a comparison of the marginal oscillations of the ice sheets of the two continents, by help of which the geologic equivalency of the successive glacial and interglacial deposits of Europe and America has been determined.

A peculiarity of the American deposits is the peripheral moraines common to, and produced by, two opposing or adjacent ice lobes and termed "interlobate" by Professor Chamberlin.

The correlation of moraines of recession with glacial lake shorelines will be noted below. In the study of the successive moraine belts between Cincinnati and Mackinac, F. B. Taylor has reached the conclusion that these moraines were produced by periodic oscillations of climate, so slow as to be referable only to some astronomical cause, probably the precession of the equinoxes. But Croll's hypothesis is not believed to furnish alone a satisfactory explanation.

As regards the precise manner of morainal accumulation, especially of the taller hills, there is not entire concordance of opinion. In several writings Mr. Upham has argued that the moraines were formed from englacial drift during episodes of equality of advance and melting, following stages of greater ablation and concentration of superglacial drift; and that these conditions were possible only during the warm Champlain epoch. Other writers deny the existence of any large amount of englacial drift, except near the base (see page 270) and regard even the highest moraine hills as due to piling of subglacial or basal drift by the thinner ice edge overriding its own debris.

Eskers.—The name "osar," borrowed from European literature, was applied by Edward Hitchcock to masses of water-laid drifts as early as 1842, and appears frequently in subsequent writings upon the drift. The word was generally used without close discrimination, however, being applied to irregular mounds of

gravel and sand as well as to ridges. An early identification was made by C. T. Jackson, in 1843, who said that the European osars were identical with the "horsebacks" of Maine. During subsequent years down to 1880 the word "kame" was generally employed instead of osar. The name "serpent kame" was applied by Shaler in 1888, in print.

The term "esker" was first used by G. H. Kinahan in 1863, being applied to gravel ridges in Ireland, but in America it remained for W J McGee to make, in 1881, the needed differentiation between eskers, applied to elongated ridges of gravel and sand, and kames, designating sand or gravel deposits of irregular or morainic topography. Gradually the word esker has displaced the less convenient word osar, although the latter has priority.

Since the acceptance of the glacial theory it has generally been recognized that these ridges of gravel, under whatever name, were formed by glacial streams. In 1872 N. H. Winchell attributed the gravel ridges observed by him in Ohio and Minnesota to the work of streams in longitudinal crevasses, and a more definite explanation in America was made by Warren Upham in 1876, when he referred those in New Hampshire to glacial rivers, either supraglacial or subglacial.

The genetic distinction between eskers and kames, made by McGee in 1881, was emphasized and amplified by Chamberlin in 1883 and 1884, referring the esker phenomena (using the term osar) to the class of axi-radiant or longitudinal stream drift, as distinguished from the peripheral kame deposits. That eskers are constructive forms, deposited in the beds of glacial streams, in comparatively stagnant ice, seems to be universally admitted. There remains, however, some lack of unanimity as to the attitude of the streams, whether upon or within or beneath the ice body. In 1884 Professor Shaler gave reasons for believing them to be formed in channels of subglacial streams. Mr. Upham has regarded the streams as supraglacial after the subglacial channels had become obstructed, or in some cases as flowing in deep ice-walled canyons, reaching perhaps to the ground. Down to 1890 George H. Stone, in his writings upon the drift phenomena of Maine, regarded the esker streams as mainly superglacial, but in 1893 he thought them subglacial, at least in the coastal region. In a critical study of the origin of certain eskers in Massachusetts by W. M. Davis, and in a discussion of the mechanics of the phenomena

by J. B. Woodworth, both authors conclude that the streams were subglacial; and the weight of opinion now favors subglacial streams in the relatively stagnant submarginal portions of the ice sheet. The observations of Professor Russell in Alaska and of Professor Chamberlin upon the ice foot in Greenland, give force to this view. There seems to be no better explanation for the greatly extended, continuous eskers of uniform cross section, or prism, which traverse hills and valleys, than that they were formed in tunnels in or beneath the ice; the streams in many cases being under hydraulic pressure. But doubtless some of the broader or shorter, discontinuous, irregular ridges may have been deposited in other ways, near the ice front. A question of some importance in this connection is the relative amount of interglacial or superglacial drift. In an ice body with little debris above its base only subglacial streams could acquire great burden.

In northeastern Iowa occur some unique deposits which may be regarded as allies to eskers. They have been described by McGee and named "paha." These are elongated ridges of aqueo-glacial material upon the surface, deposited in canyons in the ice. "The streams were originally superglacial, so that the modern drainage is 'superimposed from ice' to use Gilbert's expression; these streams were originally located by eminences under the ice, which retarded the flow and developed incipient lobation; and after the streams had deepened their channels so far as to materially reduce the thickness of the ice, then the subglacial water was forced toward the same lines, and the superglacial and subglacial flow coincided and eventually formed ice canyons in which the paha were accumulated."

Kames.—In an historical way it is difficult to discuss kames apart from eskers, as in the literature down to 1881 the several names employed were used quite indifferently and the two forms of deposits were not discriminated.

Edward Hitchcock in 1847 evidently described areas of kames under the name of "moraine terrace," and admitted that he was unable to determine their origin.

As stated above (see under eskers) the name was first used discriminatively by McGee in 1881, and Chamberlin in 1883 and 1884 described them as constructional forms, not erosional as some writers had thought, and grouped them genetically with water-laid drift, peripheral to the ice body and associated with terminal moraines.

As to the precise physical conditions under which kames, kame-areas and kame-moraines were formed there is still some uncertainty. That they were deposited by glacial streams in immediate relation to the ice edge is regarded as quite certain. But were they formed on open ground, or in standing waters? And were they from subglacial drainage or from streams higher in the ice sheet? The latter question is partly dependent upon the amount of interglacial and superglacial debris. Mr. Upham has favored the latter source of supply, but from the study of the Greenland glaciers Chamberlin thinks that both kames and eskers are from basal drift, and are "products of relatively active, vigorous glaciers." It seems that the existing glaciers of Alaska and Greenland suggest subglacial origin of the kame drift, but it is conceivable that the conditions of the American continental ice sheet may have been different.

In 1884 Professor Shaler presented an argument for the formation of kames in static water by detritus-burdened subglacial streams issuing from the ice front under hydraulic pressure. This theory might explain the majority of kame deposits, which with highly accentuated topography seem more abundant in localities of marine or lacustrine submergence during the ice recession. But mounds of water-laid drift are found at various altitudes and such were evidently formed under different conditions. It might be well to discriminate here and restrict the word kame to the typical subaqueous deposits and find another term for the subaërial accumulations.

Kettles.—Perhaps less progress has been made in explanation of the hollows or bowls called "Kettles" than of any other glacial phenomenon. The explanation of their origin made by Edward Hitchcock in 1841, that they were produced by the melting of buried masses of ice, is still the common interpretation of their genesis. In 1859 this idea was adopted by Whittlesey in his paper on the "Drift Cavities or Potash Kettles of Wisconsin." In 1877 Professor Chamberlin mentioned four possible causes of Kettles: (1) irregularities of heaping; (2) the pushing of one drift ridge unconformably against a preceding one; (3) the incorporation of ice blocks; and (4) under-drainage. In 1884 Dana thought the basins in the gravel terraces of the New Haven region were produced by eddies in the flowing water; but these have been examined by J. B. Woodworth who pronounces them typical

ice-block kettles. Those irregular depressions, sometimes of large size, which occur in water-laid drift, suggesting the name "pitted plains," have received no other acceptable explanation than that of ice-block genesis, although still regarded by many geologists as of uncertain origin.

It seems probable that "kettles" may be of various origin. Some of those in moraine and kame deposits may be due to irregular piling of the drift, while others were most certainly occupied by ice blocks during the deposition of the drift. Some of those in delta terraces are evidently due to deficient filling by the capricious action of shore and stream currents. The larger and deeper ones in river terraces or in deltas, giving rise to the name "pitted plains," are most probably of ice block origin.

The literature of the subject is scanty. Among later writers are Mr. Upham and Professor Woodworth. The latter has endeavored to estimate the size of the ice blocks from a study of the kettles.

Valley Drift, Terraces.— The enormous quantity of water-laid drift in the stream valleys leading south from the glaciated areas, as well as within those areas, was the firmest basis for the diluvial hypothesis of the drift; and the early literature contains considerable matter upon the subject. Down to 1857 the most voluminous writer was Edward Hitchcock. As early as 1833 he explained the Connecticut river terraces by a down cutting of the river through beds deposited in higher stages of flood. In his paper of 1857 he recognized the complexity of forces and thought that the valley terraces were formed in different ways; those of the Connecticut valley chiefly by a slow lifting of the land with local changes or shifting of the streams. The recognition of the valley drift of New England as derived from glacial debris and deposited by glacial floods was made by Dana in 1855. As early as 1858 M. Tuomey suggested that the Mississippi valley drift was deposited by floods from the sudden melting of the northern glaciers, and in 1859 E. B. Andrews correlated the terraces of the southern Ohio valley with the glacial drift.

Concerning details of the terrace formation in New England there have been divergence and changes of opinion. In the earlier editions of his Manual Dana held that the terrace drift was accumulated during a time of land depression and slack drainage, and the terraces excavated during pauses in the reëlevation of the

land. But in the edition of 1879 he admitted that the height of the upper terraces marked the height of the glacial flood, and that change of land altitude was not essential; thus granting the early contention of Hitchcock.

Loess.— The resemblance of certain superficial deposits throughout the interior portions of the United States to the "Loess" of Germany and of China was recognized early in this century, but the writer is not certain of the earliest suggestion of connection of the deposit with glacial phenomena. Such suggestion was made as early, at least, as 1866, by Whittlesey, who attributed the "loess-like" deposits of Illinois, southern Iowa and Missouri to floods from the melting ice sheet. From that date to the present the numerous writers upon the loess have been almost unanimous in regarding the American deposits as aqueous, and as having some relation to glacial conditions, although there were differences of opinion as to the precise conditions of deposition. One notable exception to this view was published in 1879 by R. Pumpelly advocating the eolian origin of much of the Mississippian loess. A paper by J. E. Todd before this Association in 1878 gave a good summary of the aqueous argument. In 1881 Chamberlin was almost alone in thinking that the loess of Iowa and Nebraska was partially aqueo-glacial and partially eolian, in which opinion others now concur.

During the years 1878–1881 McGee discovered that the Iowan loess was an aqueo-glacial deposit marginal to the drift sheet now named "Iowan." This work was published in 1882. This idea was subsequently amplified and given with more fullness and detail by Chamberlin.

The description by Todd and Bain, in 1895, of six feet of till, supposed to be of iceberg origin, intercalated in the loess of Iowa, would help to confirm the theory of fluvio-lacustrine origin of at least those deposits.

The latest conclusions upon the subject of the loess are found in two papers of last year, one by J. A. Udden, the other by Professor Chamberlin, which agree in attributing the Mississippian loess partially to eolian origin. The paper of Mr. Udden argues for the atmospheric origin in larger part. Professor Chamberlin holds that the loess was originally glacio-aqueous and only secondarily eolian, the latter in minor part; in these respects differing from the Asian loess of Richthofen.

Lake Basins—Preglacial Drainage.—The problem of the origin

of lake basins, especially those of the Laurentian system, has been so intimately connected with glacial studies that the subject should be mentioned.

With the extreme views of glacial erosion that were current after the general adoption of the glacial theory of Agassiz it was but natural to attribute even larger lake basins to the gouging erosion of the ice sheet. That such is the genesis of many smaller tarns and lakelets in areas of thin drift is admitted. Geikie states this emphatically for Scotland and Bell for Canada. Twenty years ago Newberry so explained the great lake basins, with Dana assenting somewhat cautiously. As late as 1894 Professor Tarr thought that the Cayuga basin had been deepened 450 feet by glacial erosion.

On account of the incidental character of the references to this matter it is doubtful where credit should be given for the earlier suggestions of the current views. As early as 1866 Whittlesey recognized that the preglacial topography of the region of the great lakes was in its broader features the same as at present. This was an important observation and gave a basis for moderate views. In 1881 J. W. Spencer attributed the great lake basins to subaërial and fluviatile agencies, and Professor Claypole the same. The complexity of their origin was emphasized by Professor Chamberlin in 1883, and this now seems evident.

In 1863 J. P. Lesley attributed the New York lakes, with Erie and Huron, to "northward rise of their floor-rock." This keen inference has been verified by studies of the old shorelines. It is now generally believed that the great lakes occupy preglacial basins of subaërial erosion, modified in some unimportant degree by the mechanical abrasion of the ice; and that the ponding of the water, while possibly due in some extent to morainal damming, is chiefly due to differential northeastward uplift.

Dr. Newberry, as early as 1862, showed the existence of ancient river channels in the Erie basin buried under glacial debris, and proving the higher altitude of the land in preglacial time. In 1869 he predicated the existence of an undiscovered channel that must have been the preglacial connection of the Huron and Erie basins, and in many subsequent writings he discussed the ancient drainage of North America. Dr. J. W. Spencer subsequently took up this work in the Laurentian basin with good results, and has endeavored to map the preglacial drainage of the great lakes.

The studies of Spencer, Carll, Foshay, I. C. White, Salisbury,

Chamberlin, Leverett and others, indicate that some of the area, now drained southward by the upper Mississippi, Ohio and Susquehanna, was in preglacial time probably drained northward.

Glacial Lakes. — The glacial phenomena receiving the latest serious study are those of ice-dammed waters. The subject has been found of unusual and romantic interest and considerable literature has accumulated, chiefly during the last decade.

The necessary existence of lakelets or small ephemeral bodies of water between the ice foot and north-sloping land surfaces has long been recognized, but it is only in recent years that the conception has been enlarged to grasp lakes of vast extent and duration. The possibility of such lakes has been questioned, but the facts are so numerous, so easily found and verified and so clear and incontrovertible that doubt is no longer possible.

With the approximate determination of the trend of the retreating ice foot, as shown by the peripheral drift, the history of the glacial waters becomes possible. The study of glacial lakes naturally followed the study of moraines. The two studies are now prosecuted together with advantage, since it is found that shorelines and outlet channels must frequently correlate with moraines.

The conspicuous shoreline phenomena about the Laurentian great lakes were early observed by settlers and travellers, particularly in the basins of Ontario and Erie, and their character as beaches was understood. Lyell in 1842 regarded them as marine, but the common explanation, and the correct one, attributed them to fresh waters. They were the subject of study and printed description by Thomas Roy (1837), Lyell, Hall, Whittlesey, Newberry, Gilbert, N. H. Winchell and Klippert before the true cause of the high waters was understood. Dr. Newberry seems to have been the first one to recognize the ice barrier as the occasion of the high-level waters in the Laurentian basin. As early as 1862 he clearly described the ice-wall of the retreating glacier as forming the northern shore of the fresh-water inland sea and described the consequent phenomena. In 1869 he presented the same facts more definitely and with greater fullness, but apparently he did not yet clearly realize that the ice wall was the dam or barrier which retained the broad waters at the ancient high level, suggesting instead some change in the attitude of the land. However, he did recognize the ice barrier in his first volume on the geology of Ohio, published in 1873; and in the second volume, 1874, he

not only clearly predicated the ice dam but discussed the whole history of the glacial waters, from the early primitive stages, in a manner that shows he had a clear conception of the main facts.

Before this Association in 1872, N. H. Winchell suggested the damming of the Michigan waters upon the north as having made the ancient Chicago outlet effective; also the blockade of the St. Lawrence valley as causing the high-level waters in the Erie basin. The latter suggestion doubtless implied the blocking also of the low divide between the Ontario basin and Mohawk valley, which is far below the old shorelines. In the same year, 1872, Winchell suggested that the waters of the Winnipeg basin (Lake Agassiz) had been forced south, through river Warren, by a glacier barrier. These were probably the earliest definite localization of glacial waters.

The first mapping of ancient beaches, and the correlation of those beaches with an abandoned outlet, was by Mr. Gilbert, in 1871, in description of the Maumee valley beaches and the Fort Wayne outlet. Land deformation he supposed was the cause of the change in water level.

The tracing of the beaches in the Erie basin was done by Gilbert, Winchell, Klippert and Newberry mostly before 1872. Newberry's map of the beaches west of Cleveland, 1874, is the first map of glacial lake beaches, so recognized.

The earliest systematic work upon the beaches in the Ontario basin was done by J. W. Spencer and published in 1882 with a theory of marine origin. In 1885 Mr. Gilbert printed a description of the high beaches upon the southern side of the Ontario basin and correlated them with an eastward outlet by the Mohawk-Hudson. The names Warren, Algonquin and Iroquois were applied by Spencer to the ancient high waters of the Erie-Huron and the Ontario basins in 1888, but with denial of their glacial character.

The earliest comprehensive account of the glacial lake history of the Laurentian basin was published by Gilbert in 1890, with maps, which were the earliest cartographic representation of glacial lakes. In this admirable paper he showed the rise of the beaches northeastward, due to the differential uplift of the region; the fluctuation of level in the basins produced by the opening of successively lower outlets toward the north and the progressive tilting of the land throwing back the flow upon southern outlets; with the final result in the present Great Lakes.

For the full confirmation of the theory of glacial dams it was necessary to prove the actual relation of moraines, as marking locations of the ice barrier, to the beaches. This was first done by Frank Leverett in 1892, in the western section of the Erie basin; and in 1895 in the southeastern section. In 1896 F. B. Taylor correlated the beaches and moraines in southeastern Michigan, thereby finding the key to the glacial lake succession in the Erie-Huron basin. His tracing of subaqueous moraines from highland to highland and their correlation with old outlets and shorelines leave no chance for further doubt of the adequacy of glacier dams.

The largest known glacial lake, and the one most fully described, is Lake Agassiz. The shoreline phenomena of this lake were described in part by H. Y. Hind as early as 1859. The southward outlet to the Mississippi was described by G. K. Warren in 1868. The glacial character of the waters was suggested by N. H. Winchell in 1872 and 1877. The name "Lake Agassiz" was applied by Warren Upham in 1879, who has immortalized the lake, and himself, by his recent monograph.

Several other investigators have described glacial lake phenomena in different localities: G. H. Cook, in New Jersey, with later description of his "Lake Passaic" by R. D. Salisbury and H. B. Kümmel; E. W. Claypole in Ohio; C. R. Dryer in Indiana; S. P. Baldwin in the Champlain valley; E. H. Williams, Jr., in the upper Lehigh valley; I. C. White in the Monongahela valley; the writer in central western New York; and especially F. B. Taylor throughout the larger part of the area of the great lakes.

The glacial lake studies have developed interesting and important results concerning crustal movements. The deformation of the shorelines gives values for the epeirogenic differential uplift over the Laurentian and the Winnipeg basins during postglacial time, of which the full significance may not yet be developed. An interesting problem now in process of solution by Taylor, Gilbert and others is the relation of glacial lakes in the upper Laurentian basin to the history of the Niagara river and the excavation of the gorge. (See above, pages 273–274.)

EXISTING GLACIERS.

The existence of living glaciers in the United States has been recognized since about 1870, and it is found that in Oregon and

Washington are alpine systems which are in some respects as interesting and instructive as those of Switzerland. The glacier fields of Alaska and adjacent Canada are far superior to those of Europe, and include the only known examples of the "piedmont" type, of Russell, the broad and comparatively stagnant field, fed by streams of the alpine type. Excepting the little known and inaccessible Antarctic area, North America possesses, in Greenland, the only existing "continental" glacier.

The first recorded observation of glaciers in the United States was made by members of the Williamson Expedition, in 1855, as Dr. Newberry stated, in writing many years later, that some of his party found miniature glaciers at the heads of streams in the group of Oregon mountains called the Three Sisters; but no description was published. In 1857 Lieut. A. V. Kautz reported the discovery of a living glacier on Mt. Rainier. In 1868 E. T. Coleman explored Mt. Baker and published in the following year a description including the glaciers. The earliest important study made by trained geologists was upon the glaciers of Mts. Shasta, Hood and Rainier in 1870, by Clarence King, S. F. Emmons and Arnold Hague.

The first geologist to examine the Alaskan glaciers was W. P. Blake in 1863, his account being printed in 1867. W. H. Dall and Marcus Baker studied the glaciers of Yukutat bay in 1874 and named the famous Malaspina glacier. Other explorers were John Muir, 1878, who discovered Glacier bay and the glacier subsequently named after him; Dall, the second visit, in 1880; C. F. Wright and S. P. Baldwin in 1886; Lieut. Schwatka and William Libbey in 1886, who gave many names to the glaciers about Yakutat and Icy bays; H. F. Reid and H. P. Cushing in 1890; and I. C. Russell in 1890 and 1891. Professor Russell discriminated and named the "piedmont" type of glacier in 1891, from his study of the Malaspina. The Copper river glaciers were noted by Lieut. H. T. Allen in 1887, and were seen by C. W. Hayes and Lieut. Schwatka in 1891.

The Canadian glaciers were first explored in the Selkirk range by Rev. W. S. Green, in 1888.

The Greenland ice foot has long been seen by voyagers, and has been written upon since 1721. The modern study began with Nordenskiold's first exploration, in 1870, and with Helland's measurements of the ice movement in 1875. The public is famil-

iar with the venturesome trips on the Greenland ice cap by Lieutenant Peary in 1886, 1892, 1894, 1895 and the trans-Greenland journey of Nansen in 1886. While adding something to glacial science, the work of these and other explorers was more particularly along lines of geographic and meteorologic science. The close geologic study only began with the work of Chamberlin in 1894. This work has been carried on by Wright, 1894; Salisbury, 1895; Barton and Tarr, 1896.

In the accumulating literature upon the living glaciers of the continent the papers by H. F. Reid and I. C. Russell, relating to the physics and phenomena of Alaskan glaciers, deserve special mention, with those of Professor Chamberlin descriptive of the structure and behavior of the Greenland ice. Two writings of Professor Russell of a general or descriptive character, bring down to date a summary of our knowledge of the glaciers of the continent; the first on "Existing Glaciers of the United States," 1881, and the second a book of the past year, entitled "Glaciers of North America."

Down to a few years ago our knowledge of glacial physics was almost entirely derived from European study. However, the glaciers of the Alps gave but small or unsatisfactory help toward the explanation of some of the most important phenomena produced by the continental ice sheets, for example, the general sheet of till, drumlins, and the various aqueo-glacial deposits as eskers, kames, loess. The interpretations were largely inferential. More has been learned of the structure, behavior and work of our ancient ice sheets by the study of the Alaskan glaciers during the last ten years, and especially by the study of the Greenland ice cap during the last four years, than by all the study of the Alpine glaciers for the seventy years since they have been observed.

The North American continent is recognized as the theatre of the greatest display of glacial activity, not in the past only, but also in the present. It must become the Mecca of the foreign glacialist. Though much has already been accomplished the work in America has only begun, and there is large opportunity for future investigation.

About 1850 the Agassizian hypothesis became the glacial theory. Now the glacial geologists understand that the glacial genesis of the "drift" is no longer a theory but an established fact. They will do well to cease paying the deference to doubt, implied in the word

"theory," and abandon its use in connection with the casual relation of the glacial phenomena. The glacial drift is as much a scientific fact as the volcanic cone of Vesuvius. The latter is seen in process of construction just as striæ, moraines, etc., are seen in the process of formation. As well might geologists speak of the oceanic theory with reference to rock strata as longer to speak of the glacial theory with reference to glacial deposits.

PAPERS READ.

[ABSTRACTS.]

OUTLINE MAP OF THE GEOLOGY OF SOUTHERN NEW ENGLAND. By Prof. B. K. EMERSON, Amherst, Mass.

THIS paper, with maps, gave a summary of the areal geology of Massachusetts, Rhode Island, Connecticut, and parts of New Hampshire, Vermont and New York.

BASINS IN GLACIAL LAKE DELTAS. By Prof. H. L. FAIRCHILD, Rochester, N. Y.

DURING the glacial recession, the impounded high waters of the Canandaigua valley, in central New York, at one time escaped across the eastern border of the basin into the Flint creek valley, which was also occupied by a glacial lake at a lower level. The river thus formed cut a channel in drift and rock and deposited the debris, as a delta, at its mouth in the lower lake. The delta now forms a conspicuous plateau of gravel 125 feet above the village of Potter.

In this plateau is an irregular depression which reaches to the very base of the delta deposit, and occupies perhaps one-fourth of the area of the plateau. The only satisfactory explanation of its origin is that an isolated block of ice was left here by the receding ice-front, and that the delta material was piled around it, the subsequent melting of the block producing the cavity. Elsewhere shallow basins occurring in deltas are in many cases attributable to deficient filling by capricious currents and wave action; but such bowls cannot be confounded with the Potter kettle-hole, which was illustrated by a map and photographs.

AN EXHIBITION OF THE RARE GEMS AND MINERALS OF MT. MICA. By Dr. A. C. HAMLIN, Bangor, Maine.

THE HUDSON RIVER LOBE OF THE LAURENTIDE ICE-SHEET. By Prof. C. H. HITCHCOCK, Hanover, N. H.

THE glacial drift and striæ of Quebec, New England, and New York, prove the existence of a glacial lobe following the Champlain-Hudson valley. The movement was to the southeast over the summits of the White and Green mountains and to the southwest over the Adirondacks, but due south along the medial valley. Last October the author climbed Orford mountain, which rises northwest of lake Memphremagog to an altitude of about 5,000 feet above the sea, and found it glaciated from bottom to top, wholly in a southeasterly course. All over the mountain were found boulders of Laurentian gneiss, which (according to their determination by Prof. Frank D. Adams of Montreal) must have come from the north side of the St. Lawrence river. It had before been shown that the highest mountains of New Hampshire and Vermont were glaciated from the northwest, but doubt had been lately expressed about Orford mountain. These observations prove that the Laurentide ice-sheet overrode all these mountains, flowing from the region north of Montreal and Quebec southward and southeastward to the sea border.

THE AGE OF THE AMBOY CLAY SERIES AS INDICATED BY ITS FLORA. By Dr. ARTHUR HOLLICK, Columbia University, New York City.

INVESTIGATIONS in the paleobotany of the Amboy Clay series of New Jersey and the equivalents on Staten Island, Long Island, Block Island, and Martha's Vineyard, conducted during the past twenty years by the late Dr. J. S. Newberry, Dr. Lester F. Ward, Mr. David White, the writer, and others, have shown that the formation which includes them is very closely related to the Atane and Patoot beds of Greenland, the Dakota group of the west, the Albirupean series of the south, and the Cenomanian of Europe, so that there was no hesitation in declaring them all to be Middle Cretaceous in age. This conclusion seemed to be quite generally accepted and was apparently not questioned until about two years ago, when an announcement was made, with some show of authority, that the series is probably Jurassic in age.

In regard to the correlation of the series with the several formations previously mentioned, there can be no question. The large amount of paleobotanical material available for comparison has given opportunity for the identification of so many species common to them all, that this conclusion is not only justifiable but inevitable; and the only question is whether this correlation also demonstrates that the several formations are Middle Cretaceous in age. If any one of them is, then they all are; if any one is not, then the others are not.

In paleobotany, as in paleozoölogy, the broad general facts are recognized that the biological sequence is coincident with the geological sequence, and that the further back in geologic time the simpler and lower in the scale of life were the organisms. Hence if we divide our fossil flora into the three great classes of cryptophytes, gymnosperms and angiosperms, the sequence of their appearance and periods of maximum development would be in the same order. The percentages of these classes in any floras should therefore be a fair indication of the relative ages of the floras. A typical Jurassic flora, such as that of Siberia, contains, roughly, the following percentages: cryptophytes, 22; gymnosperms, 74; and angiosperms (?),4. The Older Potomac flora, which is regarded as Lower Cretaceous, contains the same classes in the percentages of 39, 39, and 22; the Newer Potomac, regarded as Middle Cretaceous, 8, 13, and 79; the Amboy clays, considered separately from the latter, 6, 13, and 81; the Dakota group, 1, 5, and 94. Similar examples of percentages may also be calculated for other floras regarded as of Cretaceous age. The main fact, which is at once seen, is the manner in which the percentages of the gymnosperms and angiosperms are reversed. Few angiosperms, and only those of a doubtful character, have been found in any formation recognized as Jurassic, so that when it was ascertained that in the Amboy Clay flora and its equivalents the angiosperms represent from 70 to 90 per cent of the entire flora, there was little hesitation in considering it as well advanced in the Cretaceous period. There would be nothing inconsistent in regarding the lowest of the Older Potomac strata as Jurassic, but even there it would require definite paleontologic evidence; while in regard to the Amboy Clay series it is safe to say that a Jurassic fauna will never be found in connection with its flora.

In face of the direct evidence of the fossil flora, therefore, it would seem a very hazardous undertaking, without ample evidence in rebuttal, to draw the line of separation between the Jurassic and Cretaceous so that in the west the base of the Cretaceous would be represented by the Dakota group and in the east by the clay marls of the Matawan formation.

SOME FELDSPARS IN SERPENTINE, SOUTHEASTERN PENNSYLVANIA. By Prof. T. C. HOPKINS, State College, Pa.

THE paper described the occurrence of some large feldspar deposits, of commercial importance, in serpentine rocks in southern and central Chester county, Pa. In southern Chester county the feldspar occurs as orthoclase, associated with some muscovite, a very little biotite, considerable green hornblende (?), and, along the contact of the feldspar and the serpentine, deposits of talc and green chlorite. There is no quartz and no pegmatite. In the central Chester county area the feldspar occurs in the borders of a small serpentine area, which includes corundum mines. The spar appears

to be of the same character as that which occurs in veins in the mica schist near by. A small quantity of pegmatite was observed in this locality. The associated minerals are biotite in large quantities, muscovite in small quantities, chlorite, and a little quartz and tourmaline. No theory was put forward to account for the presence of the feldspar veins in the serpentine, pending further investigations by the author.

THE REGION OF THE CAUSSES IN SOUTHERN FRANCE. By Rev. HORACE C. HOVEY, D.D., Newburyport, Mass.

LOFTY tablelands in the departments of Lot and Lozére, along the western declivity of the Cevennes mountains, are known as the Region of the Causses. The term "Causse" is derived from the Latin word calx, meaning limestone. Some of the finest roads in Europe run along the plateaus, and occasionally descend into the valleys. But the author's exploration, here noted and illustrated with lantern views from photographs, led by the distinguished cave-hunter, E. A. Martel of Paris, left all beaten paths at the village of St. Enimie, and in canoes followed the winding gorges of the Tarn for forty miles, and then by mules or in carriages examined the gorges of the Jonté and Dourbie. The Causses vary in height from 1,000 to 5,000 feet above the sea, and these gorges are cut through them somewhat as the Grand Canyon of the Colorado cuts through the plateaus of Arizona. The cliffs of the river Tarn are often from 1,000 to 2,000 feet high, and occasionally still higher, and are brilliantly colored. On arriving at the Pas de Soucy, the Tarn suddenly sinks amid enormous rocks, with a roaring that can be heard for miles. Here a portage was made, as also around some of the waterfalls.

The caverns of the region are as remarkable as any in Europe. There are hundreds of them, and of all sizes. Among the large caverns explored by this party may be mentioned those of the Baumes Chaudes. These are three in number. From one of them the late Dr. Pruniéres exhumed three hundred prehistoric skeletons; and in another are nine vertical pits from 40 to 127 feet deep. Another cave destined to become famous is that of Darjelan, with twenty halls from 65 to 600 feet long, the lowest of them being 420 feet deep. The author's party discovered and explored the Aven Armand, down whose chasm Louis Armand was the first to go. This vertical pit is 240 feet deep, beyond which is another 300 feet deep, approached by a steep descent; the total vertical depth being 680 feet by actual measurement. The descent was made by a series of rope ladders, and was not without its dangers. The stalactitic decoration of these caves is remarkably fine.

The term "aven" is applied to what we call a "sink-hole," only the avens seem to pass more abruptly into pits or chasms. They pierce the Causses from the summit to the drainage level, and are death-traps for

animals, whose remains are found below in various stages of decomposition, and whose bones lie imbedded in the dripstone. The theory is that every aven has a passageway to the rivers of the region. That this is often so is proved by the great springs at the base of the cliffs of the Tarn; but in some cases the passageways trend away from the streams instead of toward them, and often they are dry, showing that the drainage must have been at some remotely ancient period.

A wagon accompanying this exploration carried tools, rope ladders, tents, canvas boats, and stores of provisions, and was hauled along the summit of the plateaus to meet the party when they climbed up from the canyons. The need of such varied appliances was evident on coming to explore the subterranean halls of the Bramabiau. A brook called the Bonheur flows from Mount Aigoual and bores a straight tunnel 40 feet high, 60 feet wide, and 250 feet long, to a place where the roof has fallen in, thus opening a funnel-shaped way to the surface; but the brook flows on under the debris, with roaring cascades and down gurgling gulfs, through which M. Martel made his way for 2,600 feet, emerging safely at the outlet, where the stream makes a plunge of thirty-seven feet, in a gorge 800 feet deep. It was estimated that in all its ramifications the Bramabiau has about four miles of subterranean passageways.

Besides cave explorations, the party visited an extraordinary rock-city called Montpellier le Vieux, at the junction of the Jonté and the Dourbie formed by the weathering of the Causse Noir. It has streets, fortifications, temples and palaces, more curious than the formations in the Garden of the Gods or Monument Park. It covers in all about 2,000 acres. The dolomite of the Causse Noir is of unequal hardness, causing this peculiar work of erosion.

Should it be asked why the wonderful region of the Causses has so long escaped exploration amid a country of high antiquity, the answer is that these lofty plateaus are barren solitudes, except for the chalêts of wandering shepherds. The gorges and avens have been objects of dread, instead of places attracting visitors. The superstitions of the peasants have also operated to make them shun what tourists now delight to explore. Under the stimulus of the Société de Spéléologie, the region is being opened to the public; and it is destined to be resorted to by thousands of tourists when its interesting features become more widely known.

THE WASHINGTON LIMESTONE IN VERMONT. By Dr. C. H. RICHARDSON, Hanover, N. H.

THIS name is proposed by the author for the more calcareous member of the Calciferous mica schist of Prof. C. H. Hitchcock, which in Washington, Vt., is sufficiently pure and compact to be catalogued as a crystal-like marble of great economic value. The non-calcareous member of the early Calciferous mica schist the author designates as the Bradford schist,

because in Bradford, Vt., this foliated mica schist predominates, it is the material of Wright's mountain in Bradford and Newbury. In Massachusetts the calcareous member is known as the Conway schist, and the non-calcareous as the Goshen schist of Prof. B. K. Emerson.

Since 1895 the author has mapped the line of separation between these terranes through Vermont, finding the Bradford schist flanked on the east by a band of clay slate and on the west by the Washington limestone, which in turn is flanked on the west by a band of clay slate. The two bands of slate, the Bradford schist, and the Washington limestone, lie unconformably both on the east and west in a synclinal trough of the hydromica schist, which is Huronian. There is also a striking nonconformity between the planes of deposition and the fissility, in both the clay slates and the Bradford schist, and these features have been distinctly photographed in several instances.

The eastern member of the slate was formerly supposed to terminate suddenly in North Thetford; but it is now traced continuously through Fairlee into Bradford, where it has been wholly eroded; but in the northern part of the township and in Newbury it reappears.

The Washington limestone is in general a dark gray silicious rock. The coloring matter, widely disseminated, is uncombined carbon. In the southern and western portions the limestone is darker and contains many beds of plumbaginous mica schist. In Topsham it closely resembles the Columbian marble of Rutland, Vt. In Washington and Topsham more than twenty quarries have been opened since the discovery of the marble five years ago. The rock is susceptible of a higher polish and capable of being cut to a finer edge than any other marble known to the author; and the polished letter stands out in such bold relief, with the white hammered background, that it is legible at a greater distance than the inscription upon any granite.

Its chemical analysis revealed the presence of a remarkably large number of elements, as follows: SiO_2, 35.748 per cent.; TiO_2, .189; CO_2, 22.870; Fe_2O_3, .010; Al_2O_3, 6.112; FeO, .941; GlO, .315; MnO, .075; BaO, .210; CaO, 27.304; MgO, 3.244; Na_2O, .187; K_2O, .064; Li_2O, .824; H_2O, .107; P_2O_5, 1.359; Cl, .307; F, .026; C, microscopical trace; total, 99.892: less O = Cl and F. .079; final result, 99.813.

This limestone extends through the entire length of the state and varies from 2,000 to 5,875 feet in thickness. It is no longer replete with fossils; but a few crinoidal stems are found in it at Derby, Vt., and many much larger at Wait's River. The most abundant paleontological evidence is in the Canadian territory, at Willard's Mills, Castle Brook, Magog, in Quebec, where many species of graptolites occur in slates that are probably of the same formation as the roofing slates of Montpelier and Northfield, Vt.

According to this evidence, the clay slates, which have long been regarded as Cambrian, must be Lower Silurian; and the Bradford schist and Washington limestone, which have oscillated from the "Primitive" of Zadock Thompson to the Niagara of Professor Dana, are Lower Silurian, and, more definitely, Lower Trenton.

FLUCTUATIONS OF NORTH AMERICAN GLACIATION SHOWN BY INTERGLACIAL SOILS AND FOSSILIFEROUS DEPOSITS. By WARREN UPHAM, St. Paul, Minn.

FROM a comparison of our continental drift deposits with the present retreatal conditions of the piedmont Malaspina glacier in Alaska, it is concluded that the flora and fauna adjacent to the retiring ice-sheet were nearly like those of the same latitude to-day, and that fluctuations of the ice border to the extent of a few miles, a few score or a few hundred miles, at different stages of the Ice age, due to moderate secular climatic changes, more acceptably account for our interglacial beds, former surface soils, and leached subsoils, than a general departure and renewal of the ice-sheet.

No more surprise need be occasioned by the occurrence of remains of warm temperate floras and faunas in these beds than we must feel in seeing tropical and temperate plants and animals at the foot of the Himalayas and the Alps. These extensive mountain ranges, frigid and largely snow-covered, doubtless exert as much influence on the climate of the contiguous valleys and lowlands as could be due to the waning ice-sheets of North America and Europe, when the Early Glacial high continental altitude was succeeded by the Late Glacial or Champlain depression of these great areas somewhat below their present level. The ice-sheet of each of these continents, in its time of retreat, being wasted by a warm climate at its edge, probably rose to an altitude of 5,000 feet above the land within 100 or 200 miles back from the ice border, which, therefore, might considerably readvance during any series of exceptionally cool years, with plentiful snowfall.

TIME OF EROSION OF THE UPPER MISSISSIPPI, MINNESOTA, AND ST. CROIX VALLEYS. By WARREN UPHAM, St. Paul, Minn.

UNTIL the Ozarkian epoch of great elevation of the northern part of this continent, inaugurating the Quaternary era, the upper part of the present Mississippi basin, above the vicinity of Dubuque, appears to have have been drained northerly, according to recent studies by Hershey (Am. Geologist, vol. xx, pp. 246–268, Oct., 1897). After the Cretaceous marine submergence of the state of Minnesota, its chief river system probably flowed through the Red river valley to Hudson bay during the Tertiary era, being reversed to take nearly the course of the Minnesota and Mississippi rivers at the end of that era. The St. Croix river is thought by the author to have obtained its passage through the rock gorge of the Dalles at Taylor's Falls, Minn., so late as the Buchanan interglacial epoch, preceding the Illinoian and Iowan glacial readvance; and the channel of

the Mississippi from Minneapolis to Fort Snelling, eroded during the Postglacial period, has afforded to Prof. N. H. Winchell his well-known estimate of that period as between 7,000 and 10,000 years.

SUPPOSED "CORDUROY ROAD" OF LATE GLACIAL AGE, AT AMBOY, OHIO. By Prof. G. FREDERICK WRIGHT, Oberlin, Ohio.

THIS paper details the discovery of a series of logs lying side by side as in a corduroy rod, and extending for a distance of two hundred feet or more, which were covered by thirty feet of gravel, in which were found the tooth and tusk of a mammoth, the tusk being ten feet long, twenty-two inches in circumference at the base, and weighing one hundred and fifty-five pounds. The resemblance to a corduroy road was indeed very striking; but the appearance of the logs showed that they were driftwood, and had been buried by the accumulation of the gravel that took place along the old shore of lake Erie, when, during the closing centuries of the glacial period, the water was held up to a level 150 feet higher than now. The logs and base of the deposit are 140 feet above the lake and about four miles back from it, on the banks of Conneaut creek, in the extreme northeastern corner of Ohio. The gravel was evidently brought down from the higher land to the south, near the sources of the creek, and was deposited with the mammoth remains in a delta at the edge of this old glacial lake.

In connection with this investigation, it was ascertained that similar deltas of gravel characterize the margin of the old lake where other streams from the south met it at various places between this point and Cleveland. Altogether these observations give a very vivid picture of the rapidity with which coniferous forests proceeded to cover northern Ohio as the ice melted back, and of the promptness with which the immense animals of the time reöccupied the territory. Important inferences are also derived, showing that the period of time during which the water remained at the high levels of the old ice-dammed lakes was short.

CHANGES IN THE DRAINAGE SYSTEM IN THE VICINITY OF LAKE ONTARIO DURING THE GLACIAL PERIOD. By Dr. M. A. VEEDER, Lyons, N. Y.

THE paper noted sections of wells in buried river channels south of lake Ontario, from the Niagara river eastward to the Mohawk valley.

RECENT SEVERE SEISMIC MOVEMENTS IN NICARAGUA. By JOHN CRAWFORD, Managua, Nicaragua.

DESCRIPTION of a series of earthquakes in western Nicaragua on April 29 to May 12, 1898, as reported by the author in the July number of the American Geologist (vol. XXII, pp. 56–58).

ANOTHER EPISODE IN THE HISTORY OF NIAGARA RIVER. By J. W. SPENCER, Washington, D. C.

THIS paper is a sequel to one read before the American Association four years ago on the duration of Niagara falls. It announces the discovery that while the falls were receding from Foster's flats to the locality of the railway bridges, the fall of the river reached its maximum amount of 420 feet by the retreat of the Ontario waters toward the north; and that, during the later part of this recession of the falls, past the Whirlpool rapids, the return to the present amount of 326 feet descent was interrupted by the rising of the level of the lake in the gorge to a height of 75 feet above its present level, thus reducing the actual fall of the river to 250 feet. The evidence of this is preserved in the remains of a terrace deposit opposite the foot of Foster's flats and a corresponding terrace just outside the mouth of the gorge; and these terraces, with other parts of the shore line in the Ontario basin which marks the rise of the waters so as to flood the Niagara gorge, are here named the Niagara strand.

The rising of the waters was occasioned by the lifting of the barrier at the outlet of Lake Ontario to an elevation 100 feet higher than now. By the subsequent erosion of this barrier, which was partly composed of drift, the actual fall of the Niagara waters has been increased to its present figure.

The reduction in the descent of the river is found to be sufficient to account for the shallowness of the channel at the Whirlpool rapids. The narrowness of this section is explained by the fact that the youthful Niagara took possession of a small preglacial valley there, giving greater depth to the river. It is further probable that the volume of the river was less at that time, since it is supposed that a portion of the outflow of the Great Lakes passed to the Mississippi.

THE AGE OF NIAGARA FALLS AS INDICATED BY THE EROSION AT THE MOUTH OF THE GORGE. By Prof. G. FREDERICK WRIGHT, Oberlin, Ohio.

THE late Dr. James Hall early noted the significant fact that "the outlet of the chasm below Niagara falls is scarcely wider than elsewhere along its course." Clearly this is important evidence of the late date of its origin,

and it has been used by the author and others in support of the short estimates which have been made concerning the length of time separating us from the Glacial period. A close examination made by the author this summer greatly strengthens the force of the argument, since he found that the disintegrating forces tending to enlarge the outlet and to give it a V-shape are more rapid than has been supposed while the actual enlargement at the top was found by careful measurement to be only 388 feet on each side, *i. e.* the top of the gorge is now only 776 feet wider than the bottom.

The depth of the gorge at the outlet, from the top of the Niagara limestone to the river, is 340 feet. The thickness of that formation of limestone at the surface is here, however, only about 40 feet; while the soft Niagara shales underlying it are 60 to 75 feet thick. Below there is a stratum of Clinton limestone 30 feet in thickness, and below that a shaly deposit of 70 feet. The Niagara shales at this point have never been covered by talus; so that they have always been accessible to disintegration by atmospheric agencies.

Somewhat over forty years ago, a railroad was built along the face of the eastern side of the gorge, affording an opportunity to observe the rate of the disintegration. All along where a perpendicular exposure was made, the shale has crumbled away to an extent of several feet, and in some places to twenty feet. A conservative estimate of the rate of disintegration for the 70 feet of Niagara shales supporting the Niagara limestone would be one quarter of inch a year, with a probable rate of one-half inch a year. But at the lowest estimate no more than 10,000 years would be required for the enlargement of the upper part of the mouth of the gorge 388 feet on each side, which is very largely in excess of the actual amount of enlargement. Some of the recent estimates, therefore, which would make the gorge from 30,000 to 40,000 years old, are evidently extravagant, and must incorporate some error in their premises. The age of the gorge cannot be much more than 10,000 years, and is probably, considerably less.

A RECENTLY DISCOVERED CAVE OF CELESTITE CRYSTALS AT PUT-IN-BAY, OHIO. By Prof. G. FREDERICK WRIGHT.

THE principal locality in America from which museums have been supplied with specimens of celestite (sulphate of strontium) is Strontium island, two or three miles from Put-in-Bay island, in the western end of lake Erie. Just as this supply was becoming exhausted, a remarkable fissure was discovered last winter on Put-in-Bay island which is completely surrounded with very large crystals of this beautiful mineral. The fissure was penetrated in digging a well seventeen feet below the surface, and is large enough to permit the entrance of ten or twelve people at a time. It is not an ordinary cavern, but apparently is the interior of an immense "geode" lined with celestite crystals The geological formation in which it occurs is the Waterlime of the Lower Helderberg. Large deposits of gypsum occur in the vicinity.

GEOGRAPHY AND RESOURCES OF THE SIBERIAN ISLAND OF SAKHALIN. By Prof. BENJAMIN HOWARD, London, England.

SAKHALIN has a length of about 670 miles, and a breadth of 20 to 150 miles. The features which the author observed during his visits to this island in 1890 and 1896, as here described, are (1) the absence of natural harbors and reliable anchorages around its entire 1,500 miles of coast, and the reasons for it; (2) the contrast which this island, having no volcanoes, exhibits as compared with the volcanic chain of the whole Japanese group and its continuation in the volcanoes of Kamtchatka; (3) the contradiction which this island, possessing an almost subarctic climate, affords to the popular belief that latitude is the dominant factor in the determination of climates; (4) its mineral resources, especially coal and iron; (5) the immensity and density of the fish shoals in the neighboring waters; (6) the absence of navigable rivers; (7) the persistence of unadulterated life and manners in the aboriginal Ainos there, as when described nearly three thousand years ago by the oldest Japanese historian; (8) the vast numbers of medusæ (jelly-fish) along the southern coast, and the marvelous phosphorescence of the sea as observed by the author; (9) the strategic value of the island to Russia; (10) the completeness of its adaptation to its present use as a penal stronghold; (11) the present development of its agricultural and mineral resources, and its prospective self maintenance chiefly from its future fishing industries; and (12) the expediency of maintaining the spelling Sakhalin, as here used.

EVIDENCE OF RECENT GREAT ELEVATION OF NEW ENGLAND. By J. W. SPENCER, Washington, D. C.

THE paper is a description of the valley terraces in mountainous parts of New England, illustrated by sections showing that the declivities of the valleys are not by even slopes but by a succession of steps, the plains of which become terraces farther down the valley. These steps are regarded as gradation plains in the changes of the base level of erosion, and many of the corresponding terraces are hundreds of feet above the floors of the valleys. From these features it is inferred that the recent rise of the mountainous region can be approximately measured by the sum of the heights of the steps. Yet it is not inferred that the elevation need to have been from below the sea level; and consequently the gravels are not claimed to have been necessarily of marine origin.

THE OLDEST PALEOZOIC FAUNA. By G. F. MATTHEW, St. John, N. B.

THIS fauna is contained in a series of beds unconformably underlying the Cambrian system in eastern Canada and Newfoundland. The base

of the Cambrian in the former country is marked by a barren sandstone, and in the latter by conglomerates. Erosion of the lower terrace continued up to and included the time of the Paradoxides fauna. The relation of these two terraces is comparable to that of the Upper and Lower Silurian in New York or the Carboniferous and Subcarboniferous in eastern Canada.

The fauna known consists of about twenty species. It contains no trilobites either in eastern Canada or Newfoundland. Various forms of the family Hyolithidæ are the dominant types. Other gastropods allied to Capulus and Platyceras occur; also brachiopods; remains of echinoderms (cystids ?) and corals allied to Archæocyathus and Dictyocyathus. The thin limestones which occur in the upper half of the terrane are supposed to originate chiefly from foraminifera (Globigerina, etc. ?).

THE OLDEST KNOWN ROCK. By Prof. N. H. WINCHELL, Minneapolis, Minn.

AFTER allusion to the vague ideas formerly entertained of the rocks now included under the term Archean, and still surviving in some places, this paper, by a general diagram, presents the Archean as recently made out by the Geological Survey of Minnesota. With a brief description of the other members of the Archean series, the author more particularly described the so-called greenstones of this state, which he considers the bottom of the Archean scale and the representative of the original crust of the earth formed from the molten mass by the earliest consolidation. The greenstones are divisible into two parts, one igneous and the other clastic, the latter succeeding the former with a confused and apparently sometimes non-conformable superposition, somewhat as surface eruptive rocks might be superposed, in the presence of oceanic action, upon a massive of the same nature at the same place. The clastic portions of the greenstones vary to more silicious rocks, constituting great thicknesses of graywackes, phyllites, and conglomerates, and as such have been converted by widespread metamorphism into mica schists and gneisses, the alteration coming on by degrees, increasing in intensity toward centers of granitic intrusion, and toward the great areas of granite and igneous gneiss.

Such granite, and such metamorphic rocks, as a whole, have been considered the basement rock, the oldest known rocks of the country. But, following up the long-known fact that the Laurentian granite and igneous gneisses cut the schists and sedimentary gneisses and hence are younger, they are in the same way shown to be younger also than the bottom geeenstones. They occasionally penetrate these greenstones and change them to amphibolyte and pyroxene gneiss. These metamorphic schists and gneisses seem to be a representative of the sedimentary portion of the Lower Laurentian of Canada, while the igneous granite and gneisses

are as plainly a general parallel of the igneous portion of that series. It follows therefore, that the Canadian Laurentian is, as a whole, of later date than the greenstones, if the succession is the same as in the Northwest, and that the greenstones should be considered the bottom rock of the geological scale.

THE ORIGIN OF THE ARCHEAN IGNEOUS ROCKS. By Prof. N. H. WINCHELL.

THE author discusses the greenstones and the granites, these being the extremes of differentiation, or at least of difference, in the igneous rocks. Other rocks, of intermediate characters, when plainly of intrusive nature, are considered to be due to a variable mingling of the elements of the ferro-magnesian and the alkaline magmas.

The greenstones, which were discussed in the preceding paper, are supposed to represent the primeval crust of the earth. The author denies the possibility of the derivation of the alkaline magma from these ferro-magnesian rocks by any of the methods of lixiviation or of differentiation which are currently proposed by geologists who have lately discussed the origin of the igneous rocks. Accepting this ferro-magnesian crust, as the rock of the first magma of the earth, the author shows that it could not give rise to minerals consisting largely of a potash base, such as orthoclase and microcline, which constitute the greatest distinguishing element in the alkaline magma. Neither could it give rise to the preponderating percentage of silica which accompanies the potash minerals. These characters therefore must have some other source.

Field evidence and petrographic discriminations and associations were presented, which go to show that the alkaline magma is the result of aqueo-igneous fusion of the fragmentals of the Archean itself; that when deeply buried, under heat and pressure, the Archean clastics were rendered plastic, penetrating openings in the adjacent and superadjacent strata; and that when the plastic mass was not moved from its place it was simply recrystallized *in situ*. While numerous instances of such transition from clastic to igneous rock have been noted in Minnesota, there has been a careful study of but one. That was in the case of the granite and porphyry which intrude on the clastics at Kekequabic lake.

The potash is believed to have existed in the ocean itself which immediately followed the consolidation of the first crust. Such an alkaline ocean, especially if heated, would hold in solution much silica. Hence followed the precipitation of alkaline silicates, and of excess of silica; hence also the alkaline character of the schists and gneisses when its sediments were formed into rock and metamorphosed; and hence, when fused, the alkaline magma.

As to the cause of this potassic ocean, by which the great stock of the world's potash was stored in the Archean rocks, the author does not at-

tempt any explanation, further than to suggest that possibly, owing to the chemical characteristics of potassium, it remained in the earth's atmosphere until the consolidation and also the cooling of the first crust sufficiently to allow the ocean to rest upon it, and that it was then rapidly extracted by the moist, heavy atmosphere which prevailed, being carried into the sea.

JOINTS IN ROCKS. By Prof. C. R. VAN HISE, Madison, Wis.

NOTES ON SOME EUROPEAN MUSEUMS. By Dr. E. O. HOVEY, American Museum of Natural History, New York City.

RELATING to administration and methods of display.
[Published in the American Naturalist, September, 1898.]

HISTORY OF THE BLUE HILLS COMPLEX. By Prof. W. O. CROSBY, Boston, Mass.

THE complex of the Blue Hills, on the southern border of the Boston basin, is a small part of the great granitic batholite of eastern Massachusetts, inclosing isolated masses and belts of Lower and Middle Cambrian strata. The Cambrian beds are cut by a series of diabase dikes older than the granites. The granitic series, which clearly constitutes one genetically related or consanguineous group, comprises:

I. Plutonic rocks: 1. Normal granites, forming the main body of the batholite: (*a*) biotitic normal granite (granitite); (*b*) hornblendic normal granite. 2. Rocks forming the contact zone of the batholite: (*a*) diorite and basic granite (granodiorite); (*b*) fine granite; (*c*) quartz porphyry, passing into a basic, quartzless porphyry.

II. Intrusive or dike rocks not occurring as apophyses of the contact zone; (*a*) microgranite; (*b*) aporhyolite.

III. Effusive or volcanic rocks: (*a*) aporhyolite (compact and fluidal forms).

The Cambrian sediments were first strongly folded and injected by the pre-granitic diabase; and quite certainly not later than the earlier Devonian epochs they were invaded by the great body of magma from which the granitic series was developed. No trace of the floor upon which the Cambrian strata were deposited has been discovered in the region of the Blue Hills complex. It is believed that the granitic magma was, in the main, developed *in situ* by a great heat invasion or rising of the isogeotherms inaugurated by deep sedimentation and greatly stimulated or accelerated by the thickening of the supercrust by extreme plication and flowage and the transformation of mechanical energy into heat. This

heat invasion was sufficient to induce aqueo-igneous fusion in the lower portion of the hydrated zone, whereby the pre-Cambrian rocks and a considerable volume of the overlying Cambrian sediments were absorbed by the plutonic magma. During the slow refrigeration of the batholite and consolidation of the magma, it experienced both chemical and textural differentiations; but the chemical differentiation, whether of the main body of the batholite or its contact zone, was practically limited to the greatest depths and dependent upon a convectional circulation of the magma, in accordance with Becker's theory of fractional crystallization. In conformity with this view, the contact zone is composed, at successively greater original depths, of quartz porphyry, fine granite, granodiorite, and diorite, each of these contact types passing gradually downward into the normal granite. After long continued erosion had in large part removed the sedimentary cover of the batholite, crustal adjustment permitted the extrusion of a part of the deep-seated and relatively acid magma residuum, forming intrusive masses or dikes of microgranite and aporhyolite, and effusive masses or surface flows of compact and fluidal forms of aporhyolite (felsite).

This general theory of the complex closely parallels, at most points, that proposed by Dr. A. C. Lawson for the complex of sedimentary and granitic rocks in the Rainy Lake region, as the following paraphrase of the part of his summary most germane to the present purpose will show: Whatever may have been the more remote history of the granitic rocks of this region, it appears clear that the magma from which they crystallized as diorite, granite, etc., was immediately or proximately produced by the fusion of the basement or floor upon which the Cambrian strata were originally deposited. With the fusion of this floor it seems probable that portions of the superincumbent strata have also been absorbed into the general magma and reäppeared on crystallization as granite, etc. This fusion, however, only extended up to a certain uneven surface, which now constitutes the demarcation between the granitic series and the Cambrian. Above this surface or upper limit of fusion, the Cambrian formations retained their stratiform or bedded disposition and rested as a crust of hard and brittle rocks upon the magma, subject to its metamorphic influences. There is abundant evidence that while resting upon this plastic magma the crust was violently disturbed, folded, crumpled, and in places shattered. This disturbance probably served to accentuate the sharpness of the line of demarcation between the fused and unfused rocks. (Geol. Surv. Canada, Ann. Report, new series, vol. III, 1887–88, F, 139–142.)

PALEONTOLOGY OF THE CAMBRIAN TERRANES OF THE BOSTON BASIN. By AMADEUS W. GRABAU, Boston, Mass.

THE Lower Cambrian rocks are found to contain fossils at Nahant, Mill Cove, Rowley, Topsfield, and Jeffreys' Ledge. The last three localities

were discovered by Mr. J. H. Sears, who was also the first discoverer of fossils at Nahant (1887). From collections made by Mr. Sears at Nahant seven species have been identified, including four of Hyolithes. The fossils detected in the rocks of the other localities consist of indeterminate sections of Hyolithes, and a cross-section of a trilobite from Mill Cove. From pebbles and boulders found at Nahant and Cohasset by Mr. T. A. Watson, a large number of Lower Cambrian fossils have been obtained representing fifteen species.

The Middle Cambrian of Hayward's creek, South Braintree, contains the large *Paradoxides harlani*, *Agraulus quadrangularis*, and several other forms. The Upper Cambrian is represented in this district only by erratics containing Lingula and Scolithus.

DIAMONDS IN METEORITES. By Mrs. E. M. SOUVIELLE, Jacksonville, Fla.

THE PERIODIC VARIATIONS OF GLACIERS. By Prof. HARRY F. REID, Baltimore, Md.

DISCUSSION of the causes and conditions producing periodic recessions and readvances of glaciers. The records for Europe, Asia and Greenland in 1896, and for the United States in 1897, denote a general retreat, excepting a slight tendency of advance in Greenland, as reported by the author in the Journal of Geology for July and August, 1898 (vol. VI, pp. 473–476).

NOTE ON THE OCCURRENCE OF TOURMALINES IN CALIFORNIA. By C. R. ORCUTT, San Diego, Cal.

NEAR San Diego an enormous bed or vein of lepidolite (lithia mica) contains rubellite (pink tourmaline) in large amounts. As a source of lithia and potash this deposit must soon take first rank commercially. It is now being worked as an open quarry, and 1,500,000 tons of lepidolite are estimated to be available, its width where best exposed being sixty feet or more. Much of the rubellite has been distributed to museums. Tourmalines of gem quality were first found during the present year, being all of the red variety. Black tourmalines are frequent, but green tourmalines occur only sparingly, at this locality.

THE AGASSIZ GEOLOGICAL EXPLORATIONS IN THE WEST INDIES. By ROBERT T. HILL, Washington, D. C.

THIS paper gave a brief outline of the explorations which the author has made in the West Indies during the last few years, under the patronage of Prof. Alexander Agassiz, in connection with the researches which Professor Agassiz has personally carried on in studying the greater problems in the geology and zoölogy of the American tropical regions. The geologic formations, configuration, orogeny, and fossil faunas of the West Indies are so entirely different from those of the northern United States and Canada, with which they are to be correlated, that many years must be devoted to observations and studies before final results can be presented on the remarkable geologic history of the region and its relations to continental development.

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ZOÖLOGY.

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ADDRESS

BY

ALPHEUS S. PACKARD,

VICE PRESIDENT, AND CHAIRMAN OF SECTION F.

A HALF-CENTURY OF EVOLUTION, WITH SPECIAL REFERENCE TO THE EFFECTS OF GEOLOGICAL CHANGES ON ANIMAL LIFE.

Only a little less than fifty years have passed since the publication of Darwin's Origin of Species, and the general acceptance by naturalists of the theory of descent. Since 1848 the sciences of embryology, cytology, and comparative anatomy based on embryology, or, as it is now called, morphology, have been placed on a firm foundation. It is but little over half a century since the uniformitarian views of Lyell were promulgated. The cell doctrine was born in 1839; the view that protoplasm forms the basis of life was generally received forty years since; fifty years ago the doctrine of the conservation of forces was worked out, and already by this time had the idea of the unity of nature dominated the world of science.

On the fiftieth anniversary, therefore, of our Association, it may not be out of place, during the hour before us, first, briefly to inquire into the present state of evolution and its usefulness to zoölogists as a working theory, and then to dwell more at length on the subject of the effect of geological changes on animal life.

The two leading problems which confront us as zoölogists are: What is life? and How did living beings originate? We must leave to coming centuries the solution of the first question, if it can ever be solved; but we can, as regards the second, congratulate ourselves that, thanks to Lamarck, Darwin, and others, in our

day and generation, a reasonable and generally accepted solution has been reached.

Time will not allow us to attempt to review the discoveries and opinions which have already been discussed by the founders and leaders of the different schools of evolutionary thought, and which have become the common property of biologists, and are rapidly permeating the world's literature.

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4. A third school or sect has arisen under the leadership of Weismann, who advocates what is in its essence apparently a revival of the exploded preformation, encasement, or "evolution" theory of Swammerdan, Bonnet and Haller, as opposed to the epigenetic evolutionism of Harvey, Wolff, Baer and the majority of modern embryologists. On the other hand, there are some embryologists who appear to accept the combined action of epigenesis and evolution in development.

5. Attention has been concentrated on the study of variations and of their cause. Opinion is divided as to whether variation is fortuitous or definite and determined. Many now take exception to the view, originally held by Darwin, that variations are purposeless and fortuitous, believing that they are, for example, dependent on changes in the environment which were determined in early geological periods. For definite variation Eimer proposes the term orthogenesis. Minute variations dependent on climatic and other obscure and not readily appreciable causes are now

brought out clearly by a system of varied and careful quantitative measurements.

6. More attention than formerly is given to the study of dynamical evolution, or kinetogenesis; to the effect of external stimuli, such as intermittent pressure, mechanical stresses and tensions by the muscles, etc., on hard parts. Originally suggested by Herbert Spencer, that the ultimate cause or mechanical genesis of the segmentation of the vertebrate skeleton was due to transverse strains, the segmentation of the bodies of worms and arthropods, as well as of vertebrates, has been discussed by recent workers (Rider, Cope, Meyer, Tornier, Hirsch, and others). Here should be mentioned the work done in general physiology, or morphogenesis, by Verworn, Davenport and others. Also the discoveries of Pasteur, and the application by Metschnikoff and of Kowalevsky of phagocytosis to the destruction and renewal of tissues during metamorphosis, bear closely on evolutional problems.

7. A new field of research founded by Semper, Vilmorin, and Plateau, and carried on by DeVarigny, is that of experimental evolution, involving the effects of artificial changes of the medium, including temperature, food, variation in the volume of water and of air, absence of exercise, movement, etc. Also should be added horticultural experiments which have been practised for many years, as well as the results of acclimatization.

Here should be mentioned the experiments bearing on the mechanics of development (Entwickelungsmechanik der organismen), or experimental embryology, of Oscar Hertwig, Roux, Driesch, Morgan and others, and the curious results of animal grafting and of mutilations of the embryos, obtained by Born and others, as well as the regeneration of parts. The remarkable facts of adaptation to new and unfavorable conditions of certain embryos are as yet unexplained, and have led to considerable discussion and research.

8. The *a priori* speculations of Darwin, Galton, Spencer, Jaeger, Nusbaum, Weismann and others, based on the results of the labors of morphologists and cytologists, have laid the foundation for a theory of the physical basis of heredity, and for the supposition that the chromatine in the nucleus of reproductive cells is the bearer of heredity. The theory has already led to prolonged discussions and opened up new lines of work in cytology and embryology.

9. The subject of instinct, discussed from an evolutional point of view, both by morphologists and psychologists, particularly by Lloyd Morgan, has come to the front, while mental evolution has been discussed by Romanes and others.

With all these theories before us, these currents and counter-currents in evolutional thought bearing us rapidly along, at times perhaps carrying us somewhat out of our depth, the conclusion of the whole matter is that in the present state of zoölogy it will be wise to suspend our judgment on many theoretical matters, to wait for more light and to confine our attention meanwhile to the observation and registration of facts, to careful experiments, and to repeated tests of mere theoretical assumptions.

Meanwhile we may congratulate ourselves that we have been born and permitted to labor in this nineteenth century, the century which in zoölogical science has given us the best years of Lamarck's life, a Cuvier, a Darwin, a Von Baer, an Owen, an Agassiz, a Haeckel, a Spencer, and a Huxley — the founders of modern zoölogy — who have sketched out the grander features of our science so completely, that it will perhaps, be the work of many coming years to fill in the details.

GEOLOGICAL CAUSES OF VARIATION AND OF THE EXTINCTION AND RENEWAL OF SPECIES.

The most immediate and efficient cause of variation appears to be changes of environment or of the physical conditions of existence. These, besides the agencies of gravity, electricity, of the atmosphere, light, heat, cold, food, etc., comprise geological changes or revolutions in the topography of the earth's surface at different periods. The latter causes appear to have had much to do with the process of extinction and renewal of plants and animals.

While the doctrine of the effect on animals of a change of environment was suggested very early in this century and forms the corner-stone of Lamarckism, Wallace was, after De la Beche[1] and especially Lyell[2], the first in recent times, in an essay published in 1855, to call attention to this subject thus:

[1] Researches in theoretical geology. New York, 1837. p. 217. Quoted by Woodworth, p. 220.

[2] Principles of Geology. 1830–'32.

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It is a matter of history that when a new idea or principle or a new movement in philosophy or religion arises, it at first develops along the line of least resistance; the leaders of the new thought acquire many followers or disciples. Soon the latter outstrip their teachers, and go to greater extremes; modifications of the original simple condition or theory occur, and as the final result there arise schisms and differentiations into new sects. This has happened in science, and already we have evolutionists divided into Lamarckians and Darwinians, with a further subdivision of them into Natural Selectionists and Neo-Darwinians, while the latter are often loosely called Weismannians. Some prefer to rely on the action of the primary factors of evolution, others believe that Natural Selection embraces all the necessary factors, while still others are thoroughly persuaded of its inadequacy.

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Hastings, Ontario, contain worm burrows, sponge-spicules, "and laminated forms comparable to Cryptozoön and Eozoön." (Nature, Oct. 15, 1896, p. 585.)

Even allowing room for error in the correlation of these formations, and in regarding some of these rocks as no older than Cambrian, yet on the whole the result appears to be that abundant vegetation existed in Precambrian times, which was converted into graphite, while representatives of seven classes were perhaps already in existence previous to the Cambrian period.

The following lists give a comparative view of the classes of the periods in question.

Precambrian Classes.	*Cambrian Classes.*
Rhizopoda (Radiolaria).	Rhizopoda Foraminifera and Radiolaria).
Porifera (Hexactinellid Sponges .	Porifera Sponges .
	Hydrozoa Medusæ and Graptolites)
Actinozoa (Corals).	Actinozoa (Corals .
Brachiopoda.	Brachiopoda.
Annelida.	Annelida.
	Crinoidea.
	Asteroidea.
	Lamellibranchiata.
Mollusca.	Gastropoda including Pteropoda[1]).
	?Cephalopoda Orthoceras?).
Trilobita.	Trilobita.
	Crustacea.

It would seem from these data that the physical condition of the sea and atmosphere was favorable to the existence of types for aught we know quite or nearly as highly specialized as those of the same classes now in existence. Life and nature in the Precambrian went on, so far as we can tell, much as in Cambrian times. Though locally there are breaks in the continuity of geological processes, yet probably over the world generally there was a continuity of geological phenomena, and on the whole a tolerably unbroken series of organic forms.

It is obvious, however, that in the regions thus far examined, the Precambrian, whether we include the Archean or not, more than at any time since, though the land areas are by some considered to be

[1] Dr. Matthew writes me that he looks for Hyalithoid shells should be referred to Pteropoda. "Peisander quite resembles them, and has the same heavy shells and frequent habitat on rough shores, though not a true Pteropoda."

of small extent, was a period of widespread and profound changes in the distribution of land and sea. While it is generally supposed that the extent of the continental areas at the beginning of Paleozoic time was small, forming islands, Walcott is inclined to the belief that it was very considerable, stating:

"The continent was larger at the beginning of the Cambrian period than during any epoch of Paleozoic time, and probably not until the development of the great fresh-water lakes of the Lower Mesozoic was there such a broad expanse of land upon the continental platform between the Atlantic and Pacific oceans. The agencies of erosion were wearing away the surface of this Algonkian continent and its outlying mountain barriers to the eastward and westward, when the epoch of the Lower Cambrian or Olenellus zone began. The continent was not then new. On the contrary, it was approaching the base level of erosion over large portions of its surface. The present Appalachian system of mountains was outlined by a high and broad range, or system of ranges, that extended from the present site of Alabama to Canada, and subparallel ranges formed the margins of basins and straits to the east and northeast of the northern Paleo-Appalachian or the Paleo-Green Mountains, and their northern extension toward the Precambrian shore-line of Labrador. The Paleo-Adirondacks joined the main portion of the continent, and the strait between them and the Paleo-Green Mountains opened to the north into the Paleo-St. Lawrence Gulf, and to the south extended far along the western side of the mountains and the eastern margin of the continental mass to the sea that carried the fauna of the Olenellus epoch around to the Paleo-Rocky Mountain trough." (*l. c.* p. 562.)

Remarking on the habitat, or nature and extent of the sea-bottom tenanted by the Olenellus or Lower Cambrian fauna, Walcott remarks:

"One of the most important conclusions is that the fauna lived on the eastern and western shores of a continent that, in its general configuration, rudely outlines the North American continent of to-day. Strictly speaking the fauna did not live upon the outer shore facing the ocean, but on the shores of interior seas, straits, or lagoons that occupied the intervals between the several ridges that rose from the continental platform east and west of the main continental land surface of the time." (*l. c.* p. 556.)

Dana had previously (1890) claimed that the earth's features even to many minor details were defined in Archean time (evidently referring to all Precambrian time) and that "Archean conditions exercised a special and even detailed control over future continental growth." May not this idea be extended to include

the life of the Precambrian, and may we not suppose that biological variations and evolutions were predetermined to some degree at least by the geological conditions of these primeval ages? The continental masses were then foreshadowed by submarine plateaus covered by shallow seas, the deeper portions of the ocean basins not being affected by these oscillations, extensive as they were.

The time which elapsed between the end of the Laurentian and beginning of the Cambrian was immense, or at least, as long as the entire Paleozoic era. Walcott estimates the length of the Algonkian at 17,500,000 years. This length of time, or even a portion of it, was long enough for the origination and establishment of those classes, whose highly specialized descendants flourished in the Cambrian. Referring to the Precambrian strata Walcott states:

"That the life in the pre-Olenellus seas was large and varied there can be little if any doubt. The few traces known of it prove little of its character, but they prove that life existed in a period far preceding Lower Cambrian time, and they foster the hope that it is only a question of search and favorable conditions to discover it."[1]

Here the imagination of the zoölogist may be allowed for the moment free scope to act. It is perhaps not hazardous to surmise that in the early centuries or millenniums of the Huronian there arose from some aggregated or compound infusorian, the prototype of the sponges.

From some primitive gastrula, which became fixed to the Huronian sea-bottom may have arisen the hydroid ancestor of the cœlenterates; owing to its fixed mode of life, the primitive digestive cavity opened upwards, being held in place by the septa, so that the vase-shaped body, growing like a plant, with the light striking upon it from all sides, assumed a radial symmetry. Before the beginning of the Cambrian, for we know Aurelia-like forms abounded on the Cambrian coasts, medusæ budded out from some hydroid polyps, became free-swimming, and as a result of their living at the surface became transparent, and thus shielded from the observation of whatever enemies they had, multiplied in great numbers.

From some reptant gastræa there may have sprung, in these primeval times, an initial form with a fore-and-aft, dorso-ventral

[1] The Fauna of the Lower Cambrian or Olenellus zone. Tenth Annual Report of the U. S. Geological Survey, 1888–89.

and bilateral symmetry, which gave origin by divergent lines of specialization to flat-worms, nemertean, and round-worms, as well as Rotifera, and other forms included among the Vermes. It is probable that the trematodes and Cestodes, especially the latter, whose organs have undergone such reduction by parasitism, and some of which through disuse have totally disappeared, did not evolve until some time after the appearance of molluscs and fishes.

When existence in these early plastic vermian forms was confined to boring in the mud and silt, the body became cylindrical, as in some nemerteans, and in the thread-worms; some of the latter forms, boring into the mud, became parasites, entering the bodies of other animals which serve as their hosts.

At about this time certain worms, as the simple mechanical result perhaps of threading their way over or through the rough gravelly bottom, became segmented. The establishment of a segmented structure, brought about by the serpentine mode of progression in the direction of least resistance, resulted in the origination of a succession of levers. Following this annulated division of the dermo-muscular tube of worms, was the serial or segmental arrangement of the internal organs, *i. e.* the nervous, excretory, reproductive and glandular, and in a less degree, the circulatory system.

In certain of these primitive protannelids, as the result perhaps of external stimuli intermittently applied, bristles originated to aid in progression, and finally the segmentally arranged lateral flaps of the skin, the parapodia, which served as swimming organs. Other nepionic forms, at first free swimming, became fixed and protected by two valves as in the Brachiopoda, which owe their success in Precambrian times to their fixed and protected bodies.

Not long after the annelid type became established that of the echinoderms apparently diverged from some nepionic worm, like a trochosphere. In such a form there was a tendency to the deposition of particles and plates of lime in the walls of the body, and the type becoming fixed at the bottom, or at least nearly stationary, and meanwhile more or less protected by a calcareous armor, lost its originally bilateral, and acquired a radial symmetry.

But no echinoderms have yet been detected in Precambrian rocks, which however have revealed arthropods, as shown by the traces of a trilobite, and this tends to indicate that radial symmetry is an acquired, not a primitive characteristic.

At this time was solved the problem of the origination of a type of body, and of supports for it either in walking or in swimming, which should fulfil the most varied conditions of life, and this type, the arthropodan, as events proved, was that fitted for walking over the sea-bottom, for swimming, or for terrestrial locomotion; nor was the idea of segmentation both in trunk and limbs discarded when the type culminated in flying forms — the insects.

The Arthropoda, as the record shows, first represented by trilobites, which structurally are nearer the annelids than Crustacea, was destined to far outnumber in individuals, species, orders and classes any other phylum. Fundamentally worm-like or annelid in structure, the body consisted of a linear series of stiff levers, and was supported by limbs segmented in the same way. The variations of the arthropodan theme are greater than in any other groups, and nature, so to speak, succeeded most admirably in this type, with the exception of the Trilobita, which was the first class of the phylum to appear and the first to disappear. The evolution of jointed limbs was accomplished in the most economical and direct way. The parapodia were perhaps [illegible] and at first retaining the [illegible] form in swimming phyllopods, afterwards from being used as supports became cylindrical and jointed. All this modification of monotype forms and evolution from them to other types was accomplished not very late perhaps in the [illegible]. After the specialization of the antennae and of the trunk segments of the trilobites was worked out, all the postantennal appendages being alike, there ensued in some descendants of another primal ancestor a further differentiation of the postantennal appendages into mandibles, maxillae, maxillipeds, thoracic ambulatory legs, and abdominal swimming feet as worked out in the more specialized members of the class of Crustacea.

As soon as the crustacean type became established the [illegible] must have been most favorable for its rapid differentiation [illegible]

Indeed the three lines of descent which resulted in the arthropodan phylum, as it now exists, unless there were three independent phyla, were perhaps initiated before the Cambrian. These lines are (1) the Trilobita, with their probable successors the merostomes and arachnids, (2) the Crustacea, and (3) the myriopods and insects. Of the third line Peripatus or a peripatus-like form was the earliest ancestor, which of course must have been terrestrial in habits, though its forefather may have been some fresh-water leech-like worm. We venture to state that it is not wholly impossible that so composite a type as Peripatus, which bears at least some of the marks of being a persistent type, took its rise on the continental land of the Precambrian.

In the Precambrian time was also solved the problem by the molluscs of producing a spiral univalve shell; for while a large proportion of the Gastropoda were protected by patella-like shells of simple primitive conical form, with these coëxisted in the lowest Cambrian forms with spiral shells, such as Platyceras and Pleurotomaria. The comparative abundance of those highly modified molluscs, the Pteropoda, in the lowest or Olenellus Cambrian strata, strongly suggests that their divergence from the more generalized gastropod stem and their adaptation to a surface or pelagic life must have taken place long anterior to the dawn of the Cambrian.[1] With them must have lived a variety of other surface forms besides Rhizopoda, whose young served as their food. The members of all classes of the Cambrian were carnivorous, feeding on the protoplasm of the bodies of microscopic animals, or on the eggs and young of their own species, some living on the bottom, and others at the surface. Of marine plants of the Cambrian there are but slight traces, and it is evident that what there were were restricted to the coasts and to shallow water. The old idea that plants originally served as the basis of animal life must be discarded. As at present no plant life exists below a few fathoms, a hundred perhaps at the most, and since below these limits the ocean depths are packed with animal life which exists entirely on the young or the adults of weaker forms, so must the rise and progress of animal life have been quite independent of that of plants. The lowest plants and animals may have evolved from

[1] Dr. Matthew has discovered at St. John, N. B., a still lower and older bed, containing no Olenellus; but Foraminifera (Orbulina and Globigerina), sponges, Pteropoda, Pelagiella which was probably an oceanic Heteropod, very primitive brachiopods, with Ostracoda and six genera of trilobites.

some common bit of protoplasm, some protist, but the evolution of the animal types became very soon vastly more complex. The specialization of parts and adaptation to the environment were more thorough and rapid in the lowest animals evidently in consequence of the greater power of locomotion, and aggressiveness in obtaining food from living organisms, and the adaptability of animal life to various oceanic conditions, especially temperature, bathymetrical conditions and a varying sea-bottom.

This rapid differentiation and multiplication of different family, ordinal, and class ancestral types, went on without those biological checks which operated in later times, when the seas and land masses of the globe became more crowded. There was a comparative absence of competition and selection; this being due to the lack of predaceous carnivorous forms to produce that balance in nature which afterwards existed. The two most successful and abundant types were the trilobites and brachiopods; but the former were not especially aggressive in their habits, undoubtedly taking their food in a haphazard way by burrowing in the mud or sand, having much the same kind of appendages and the same feeding habits as Limulus. The brachiopods were fixed or burrowed in the sand, straining the microscopic organisms drawn into the mouth by the currents set up through the action of their ciliated arms. The most destructive and aggressive Cambrian animal known to us was the Orthoceratites, but their remains have not yet been detected below the 2d Cambrian zone. Even if some protocordate Balanoglossus, Ascidians, or even Amphioxus had already begun their existence in these Precambrian times, they could have caused but a little more destruction of life than their contemporaneous invertebrate allies. As the remains of Ostracodermi and sharks have been detected in Trenton strata, perhaps they originated in the Cambrian, when they must have been active forces in the elimination of those Precambrian soft-bodied animals which connected classes now quite wide apart.

The rapid increase in the Precambrian population was hastened by the probable fact that this, more than any subsequent period, was one of rapid migration and colonization. Vast areas of the shallow depths over the site of the embryo continent, more or less shut off from the main ocean by shoals, reefs and islands were, by oscillations of the sea bottom and land, opened up at various times to migrants from the older previously settled seas.

The nature of the Precambrian sediments shows that the more open sea-bottom was swept by tidal and ocean currents varying in strength and extent. The topography of the ocean bottom over what is now land must have been more diversified than at present. In the late ages of the Algonkian, owing to active competition and the struggle fo existence in the overstocked areas, the process of segregation or geographical isolation was rapidly effected, and the migrants from the denser centers of growth pressed into the then uninhabited areas where, as new vigorous and prepotent colonists, they broke ground and founded new dynasties.

At such times as these we can easily imagine that, besides the absence of competition, the Lamarckian factors of change of surroundings bringing about new habits and thus inducing new needs, the use and disuse of organs, together with the inheritance of characters acquired during the lifetime of the individual, operated then far more rapidly and in a much more thoroughgoing way, than at any period since, while all through this critical, creative period, as soon as there was a sufficient diversity in the incipient forms and structures, a selective principle began to operate.

For forty years past, since the time of Darwin, the idea that these early forms were more rapidly evolved, and that they were more plastic than forms now existing has constantly cropped out in the writings of our more thoughtful and studious paleontologists and biologists.

Darwin, in his Origin of Species, as quoted by Walcott with approval, remarked that it is indisputable that, before the lowest Cambrian stratum was deposited, long periods elapsed, as long as, or probably far longer than, the whole interval from the Cambrian age to the present day; and that "during these vast periods the world swarmed with living creatures." Darwin then adds: "It is, however, probable, as Sir William Thompson insists, that the world at a very early period was subjected to more rapid and violent changes in its physical conditions than those now occurring; and such changes would have tended to induce changes at a corresponding rate in the organism which then existed."

Professor Hyatt,[1] from his exhaustive studies on the Nautiloidea and Ammonoidea, concludes:

"These groups originated suddenly and spread out with great rapidity, and in some cases, as in the Arietidæ of the Lower Lias, are traceable to an origin in one well-defined species, which occurs in close proximity to the

[1] Phylogeny of an acquired characteristic. Proc. Amer. Phil. Soc. xxxii, p. 371.

whole group in the lowest bed of the same formation. These facts, and the acknowledged sudden appearance of large numbers of all the distinct types of invertebrates in the Paleozoic, and of all the greater number of all existing and fossil types before the expiration of Paleozoic time, speak strongly for the quicker evolution of forms in the Paleozoic, and indicate a general law of evolution. This, we think, can be formulated as follows: *Types are evolved more quickly and exhibit greater structural differences between genetic groups of the same stock while still near the point of origin, than they do subsequently. The variations or differences may take place quickly in the fundamental structural characteristics, and even the embryo may become different when in the earliest period, but subsequently only more superficial structures become subject to great variations.*"[1]

If this applies to the evolution of these cephalopods in the Mesozoic, how much more rapidly and efficaciously did the principle operate in the Precambrian period, after the initial steps in the divergence of types from the unicellular Protozoa took place? The same law or fact obtains with the insects, the eight holometabolous orders having, so far as the evidence goes, originated at nearly the same geological date, near or soon after the close of the Paleozoic era. Williams also shows from a study of the variations of *Atrypa reticularis* that this species in its specific characters shows a greater degree of variability of plasticity in the earlier than in the later stages of its history. We thus conclude that after the simplest protoplasmic organisms originated, the greatest difficulties in organic development, *i. e.*, the origination of the founders of the different classes were, so to speak, met and overcome in Precambrian times. The period was one of the rapid evolution of types. As Williams[2] has well remarked:

"The chief expansion of any type of organism takes place at a relatively early period in its life history. Since then as with the evolution of the continent itself, the farther progressive differentiation of marine invertebrate forms has, since the close of the Precambrian, been a matter of detail."

As well stated by Brooks, since the first establishment of the Cambrian bottom fauna, "evolution has resulted in the elaboration and divergent specialization of the types of structure which were already established, rather than in the production of new types."

In accepting the general truth of this statement, and its application to the marine or Cambrian types it may, however, be modified to some extent. For during the late Paleozoic was witnessed the

[1] Geological Biology, p. 322. [2] *l. c.* p. 347.

evolution of the three tracheate, land-inhabiting, air-breathing classes of Arachnida, Myriopoda, and insects, and of the air-breathing vertebrates, with limbs and lungs, comprising the four classes of amphibians, reptiles, birds, and mammals.

2. *The Appalachian Revolution and its biological Results.*— Unless we except the great changes in physical geography which took place at the end of the Tertiary period, when the mountain chains of each continent assumed the proportions we now see, the Appalachian revolution, or the mountain-building and continent-making at the close of the Paleozoic age, was the most extensive and biologically notable event in geological history. In its effects on life, whether indirect or direct, it was of vastly greater significance than any period since, for contemporaneous with and as a consequence of this revolution was the incoming of the new types of higher or terrestrial vertebrates. Through the researches, now so familiar, in the field and study, of the two Rogerses, of Dana, and of Hall, we know that all through the Paleozoic era at least some 30,000 to 40,000 feet of shoal water sediments, both marine and fresh water, derived from the erosion of neighboring lands, were accumulated in a geosynclinal trough over the present site of the range extending from near the mouth of the St. Lawrence to Northern Georgia. At the end of the era ensued a series of movements of the earth's crust resulting from the weight of this vast accumulation, which in a geologically brief period sank in, dislocated, and crushed the sides of the trough, and folded the strata into great close parallel folds, besides inducing more or less metamorphism. These folds rising from a plateau formed mountain ranges perhaps as high as the Sierra Nevada or Andean Cordillera of the present day. The plateau emerged above the surface of the Paleozoic ocean, and was carved and eroded into mountain peaks, separated by valleys of erosion, the rivers of the Appalachian drainage-system cutting their channels across the mountain ranges.

But this process of mountain building and erosion was not confined to the end of the Paleozoic era. Willis[1] has shown that there have been several successive cycles of denudation, covering a period extending from the end of the Paleozoic era to the present time. And it is the fact of these successive cycles of denudation both on the Atlantic and Pacific slopes of our continent that is of high significance to the zoölogist from the obvious bearings of these

[1] National Geographic Magazine, 1889, vol. 1, pp. 291–300.

revolutions on the production of variations. Indeed it is these phenomena which have suggested the subject of this address.

We can imagine that this great plateau, in the beginning of the Mesozoic era, with its lofty mountain ranges and peaks rising from the shores of the Atlantic, presented different climatic zones, from tropical lowlands with their vast swamps, to temperate uplands, stretching up perhaps to alpine summits, with possibly glaciers of limited extent filling the upper parts of the mountain valleys. New Zealand at the present day has a subtropical belt of tree ferns, while the mountains bear glaciers on their summits; and in Mexico, only about 20° from the tropics, rising above the tropical belt, is the temperate plateau, and farther up the subalpine snow-clad summits of Popocatapetl, Orizaba, and other lofty peaks. So in the Appalachians of the Paleozoic, the cryptogamous forests and their animal life may have been confined to the coastal plains and lowlands, while on the higher, cooler levels may have existed a different assemblage of life; and it is not beyond the reach of possibility that a scanty subalpine flora peopled the colder summits.

But the unceasing process of atmospheric erosion and river action continued through the Jurassic which was, as stated by Scott, in his Introduction to Geology, "a time of great denudation, when the high ranges of the Appalachian mountains were much wasted away, and the newly upheaved, tilted and faulted beds of the Trias were deeply eroded." At about the time of the opening of the Cretaceous the range was reduced to a peneplain (the Cretaceous peneplain), with only vestiges of once lofty mountains; the scenic features roughly recalling those of North Carolina and New England at present, although more subdued and featureless, more like the Kittatinny peneplain of the Piedmont district at the eastern base of the Blue Ridge to-day as contrasted with the present mountain region of Pennsylvania and New Jersey. There were also extensive changes in the interior. What was the Colorado island was added to the mainland, and a great Mediterranean sea extended from the Uinta mountains of southeastern Wyoming to New Mexico and Arizona, and stretched from the Colorado peninsula westward to Utah. In the upper Jurassic as the result of a depression a gulf was formed over northern Utah, Wyoming and Southern Montana (Scott).

The formation of this Cretaceous peneplain was succeeded by a reëlevation, and the surface which is now Virginia was gradually

raised to a height of 1400 feet, and again the sluggish rivers of the Cretaceous times were revivified, cutting through the harder strata forming the walls of the longitudinal valleys and, widening into broad estuaries, emptied into the Atlantic.

In the Eocene tertiary as Willis tells us: "The swelling of the Appalachian dome began again. It rose 200 feet in New Jersey, 600 feet in Pennsylvania, 1700 feet in Southern Virginia, and thence southward sloped to the Gulf of Mexico." In consequence of the renewed elevation, the streams were revived; and Willis adds: "Once more falling swiftly they have sawed, and are sawing, their channels down, and are preparing for the development of a future base-level."[1]

We can in imagination see, as the result of these widespread physical changes, inducing as they must have done the formation of separate basins or areas enclosed by mountain ranges, with different climates and zones on land, however uniform might have been the general temperature of the world at that time and the other physical conditions of the sea,—we can imagine the profound and deep-seated influence thus exerted on the life-forms peopling the uneven surface of the land.

The vegetation of the lowlands was rich and luxuriant, as the Triassic (Newark) coal deposits near Richmond testify, and while the uplands and hills were probably clad with dense forests of conifers, on the dryer desert areas of the peneplain the trees may have been more scanty, like the scattered pines of the dryer elevated region of the southwest, and of the Great Basin at the present day. The distribution of the animal life must have corresponded; one assemblage, especially the amphibians, characterizing the hot and humid lowlands, another the cooler uplands, while already perhaps a few forms became adapted to the more arid desert areas, as is the case now in Australia, which is in a sense a Mesozoic continent.

Similar subsidences and elevations changed the Jurassic map in Eurasia. This continent was already a land mass of great extent, and fresh-water lakes extended across Siberia, and in China were extensive swamps and submerged lands, now represented by coal fields. Afterwards in the middle Jura this continent subsided, and the Jurassic sea covered the greater part of Europe and Asia, this being, according to Neumayr, "one of the greatest transgressions

[1] Quoted from Scott's Introduction to Geology, p. 342.

of the sea in all recorded geological history." Subsidences and elevations resulted, it is supposed, in cutting off India from Eurasia so that the strait or sea covered the site of the Himalayas, and India was possibly joined to Australia, the Malaysian peninsula forming the connecting link; or perhaps it stretched to the southwestward and was joined to South Africa. However this may be, it is sufficient for our present purpose that these vast changes in the relative position of land and sea were productive of a corresponding amount of variation and perhaps of immigration and consequent isolation. At all events throughout the Jurassic seas as a whole there seemed to have been remarkable faunal differences. This led Neumayr, in which he is followed by Kayser,[1] to conceive that there were already in Jurassic times climatic zones, corresponding to the boreal, polar, north and south temperate, and tropical zones of the present day. If, however, with Scott, we reject this view, and substitute for it the supposition that "the marked faunal differences are due to varying facies, depth of water, character of bottom, etc., and even more to the partly isolated sea-basins and the changing connections which were established between them," it is of nearly the same import to the geological biologist, for these varying conditions of the Jurassic ocean-bottom could not have been without their influence in causing variation, modification and adaptation to this or that set of conditions of existence.

Turning now to the effects of the Appalachian revolution on the life of that time, we see that the biological results were, in the main, in conformity with the geological changes. During the carboniferous period vertebrates with limbs and lungs appeared, *i. e.*, the labyrinthodonts or Stegocephala. They were, compared with the other orders of their class, the most composite and highly organized of the Amphibia.

Throughout the long period of comparative geological quiet, those long ages of preparation which ended in the crisis or cataclysm which closed the Paleozoic, the amphibian type was slowly being evolved in the swamps and bayous of the lowlands of the Devonian, whose vegetation so nearly anticipated that of the carboniferous, from some Devonian[2] or late Silurian ganoids, from which diverged on the one hand Dipterus and the colossal lung-fish

[1] Text book of Comparative Geology, translated and edited by Philip Lake, pages 270, 271.

[2] Certain footprints recently discovered in the upper Devonian show that the type had become established then, at least vertebrates with legs and toes.

(Dinichthys and Titanichthys) of the Devonian, and perhaps on the other the labyrinthodonts, which may have sprung from some crossopterygian fish like Polypterus, and whose pectoral and ventral fins became adapted for terrestrial locomotion. The type evidently was brought into being provoked by, and at the same time favored by the great extent of low coastal swampy land and bodies of fresh water which bordered the Atlantic seaboard from the Silurian time on.

How the amphibian type arose from the ganoid stock is a matter of conjecture. It may however be surmised that certain of the lung-fishes or forms like them, adapted for breathing the air direct when out of the water in the dry season, instead of remaining in their mud cells waiting for the rains to fill the lakes or swell the rivers, attempted, like the Anabas, or climbing fish, to migrate in schools overland; or, like that fish, which is said to have become "so thoroughly a land animal that it is drowned if immersed in water,"[1] it may have become confined to the land, and losing its gills, used its lungs only. As the final result of its efforts to walk over the damp soil and mud of swampy regions, the uniaxial fins may have developed, through the strains and pressures of supporting the clumsy body, into props with several leverage systems; the basalia instead of remaining in one place as in a fish's fin, spreading out and becoming digits to support the weight and steady the body while walking. This process was not confined to one or to a few individuals, but as Lamarck insists in the cases he mentions, it affected all the individuals over a large area. Those individuals with incipient limbs became erased or swamped, and we find no trace of them in the strata yet examined.

Thus far, indeed, Paleontology is silent[2] as to the mode of origin of the amphibian limb, as it is concerning the origin of arthropod limbs from the parapodia of annelids. Unfortunately, and this is still a weak point in the evolution theory, nowhere do we find, unless we except the Archæopteryx, clear examples of any intermediate forms between one class and another; each species as far as its fossil remains indicate seems adapted to its environment.

[1] Parker and Haswell's Text book of Zoölogy, vol. ii, p. 220.

[2] Paleontology is also equally silent as to the origin of plesiosaurs and ichthyosaurs from their terrestrial digitigrade forebears; though in Archæopteryx we have an unusually suggestive combination of reptilian and avian features. Certain Theriodontia point with considerable certainty to the incoming of mammals such as the Echidna and duckbill, but as to the steps which led to the origin of brachiopods, echinoderms, trilobites, of Sirenians and of whales paleontology affords no indications.

There are numerous cases of vestigial structures, but no rudimentary ones showing distinct progressive steps in a change of function. Hence arises the very reasonable view held by some that nature may make leaps, and that new adaptations or organs may be suddenly produced. No inadapted plant or animal as an entire organism has ever been observed either among fossils or existing species. Man has some seventy vestigial structures, but his body as a whole, notwithstanding the disadvantages of certain useless vestiges, is in adaptation to his physical and mental needs.

While the true carboniferous labyrinthodonts were few and generalized, with gills and four legs, already in the Permian, where we meet with some thirty forms in the Ohio beds alone, and about as many in Bohemia, a great modification and specialization had taken place. Forms like Peleon and Branchiosaurus had gills and four legs; others were like our lizards, as in Keraterpeton; Dendrerpeton and Hylonomus of Nova Scotia were more lizard-like and with scales; others perhaps swam by means of paddles as in Archegosaurus; others like the "Congo snake" were snake-like with small weak legs, as Œstocephalus; some had gills but no legs, as in Dolichosoma, while in others the limbless body was snake-like and scarcely larger than earth worms, as in Phlegethontia of the Ohio and Ophiderpeton of the Bohemian coal measures.

Already, then, in Permian times the stegocephalous type showed signs of long occupation, old age and degeneration. The process of degeneration and reduction in and loss of limbs may have been initiated as far back as the closing centuries of the Devonian.

The effect of the Appalachian revolution and corresponding physical changes in Europe, was by no means disastrous to the Stegocephala, for those of the Liassic, where the conditions must have been more formidable to terrestrial vertebrate life, were abundant, and in some cases at least colossal in size. Whether the salamanders, cœcilians, sirens, and Amphiuma of present times are persistent types, survivors of Carboniferous times, or whether the process of modification has been accomplished a second time within the limits of the same class is perhaps a matter for discussion.

Besides the introduction and elaboration of the air-breathing, four-footed labyrinthodonts, the sloughs and sluggish streams were alive with Naiadites and its allies, forerunners of the Unionidæ, and with them lived shelled Phyllopods, Estheria having

already appeared in the Devonian, Leaia appearing in the Carboniferous; and also the larvæ of aquatic net-veined insects, fragments of the imagines of which were detected by Hartt at St. John, New Brunswick.

The coal-bearing strata are largely fresh-water beds of fine shale and well calculated to preserve the hard parts of delicate animals, but on general grounds it is evident that the great extent of low lands with extensive bodies of fresh water, communicating with the shallow sea, was most favorable to the development and differentiation of terrestrial life. Though fresh-water and land shells (pulmonates) appeared in the Devonian, they were apparently more abundant in the coal period. Especially rapid was the incoming of the arthropods; both diplopods, some of them very remarkable forms, and chilopods, lived sheltered under the bark of colossal lycopods; with them were associated scorpions, harvestmen and spiders. The great profusion of net-veined insects discovered at Commentry, France, shows that this was the age of the lower more generalized or heterometabolous insects, such as cockroaches, and other Orthoptera, of Eugereon, may-flies and possibly dragon-flies, etc., our wingless stick-insects being then represented by winged ancestors. At this time also began the existence of insects with a complete metamorphosis, as traces of true Neuroptera and the elytra of a beetle have been detected in Europe. But thus far no relics of flowers or of the insects which visit them have been discovered in Carboniferous times, not even in the Permian, so that the origin of insects with a complete metamorphosis, such as moths, ants and flies, may be attributed to the new order of things, geographical and biological, immediately following the Appalachian revolution.

We do not wish to be understood as implying that the origin of new orders and classes is directly due to geological crises or cataclysms themselves.[1] On the contrary the initial steps seem to have

[1] I find that Wood has already expressed the same idea more fully, as follows: "Both in the palæozoic and secondary periods, therefore, the complete changes in the fauna which marked their termination do not appear to have been immediate upon the changes of the geographical alignement, but to have required the lapse of an epoch for their fulfilment; and the completeness of that change is perhaps not less the indirect result of the altered alignement, by the formation of continents where seas had been, and the opening out of new seas for the habitation of marine animals, thereby causing a gap in the geological records so far as they have been hitherto discovered, than the direct result of the changed conditions to which the inhabitants of the seas, and even those of the land, came to be subject on account of the entire change in the alignement of the land over the globe." (Phil. Mag. xxiii, 1862, p. 261.)

been taken as the result of the gradual extension of the land masses, and the opening up of new areas; it was the period of long preparation, with long-continued oscillations, the slowly induced changes resulting from the reduction of the mountainous slopes to peneplains, which were most favorable to the gradual modification of forms resulting in new types, the gradual process of extinction of useless and senile forms, and the modification and renewal of those which became adapted to the new geographical conditions.

It should be borne in mind that this extension of the low coasts of the continents began in Ordovician times, but the remarkable expansion of our continent after the Appalachian revolution, rather than the upheaval of the plateau itself, so favorably affected plant and animal life, that at the dawn of the Mesozoic a great acceleration in the process of type-building was witnessed. Moreover, it seems evident that the variation which took place at this epoch was by no means fortuitous, but determined along definite lines caused by the definite expansion of the continents, and their resultant topography.

We have seen that as a result of the folding and upheaval of the Appalachians, there may have been at the beginning of Triassic time in addition to the tropical lowlands, a somewhat cooler upland zone, and possibly even snow-clad mountain peaks, with glaciers descending their sides, as we may now witness in New Zealand.

Already on Permian soil reptiles were not infrequent. They were generalized composite forms comprising the Proganosauria, the forerunners of the Hatteria of New Zealand, and the Theriodontia, from which the mammals are now supposed to have been derived. They disappeared at the end of the Triassic, together with the labyrinthodonts, from which the reptiles are thought to have originated. These reptiles having scaly bodies and claws, their habits must have been like those of the lizards of to-day, and they were adapted for hotter and dryer, perhaps more elevated areas than the stegocephalous amphibians; and these conditions were fulfilled in Triassic and Jurassic times, when the reptilian orders multiplied, all the orders of the class having been differentiated in the Mesozoic era, if not before.

The geographical features throughout the Mesozoic were these: more or less dry and broad plains, vast fresh-water lakes, uplands

clad with coniferous forests afterwards to be replaced by forests of deciduous trees; flower-strewn plains overgrown with waving grasses, and jungles with rank growths of bamboo. We can, without going into detail, well imagine that the geographical feaures of the mesozoic continents were such as to provoke the appearance of the higher classes of vertebrates. As the land rose higher and the low swampy coastal areas became more limited, this would tend to restrict the habitat of the stegocephalous amphibians; with a slightly more elevated and dryer coast, the incoming and expansion of reptilian life were fostered; with still higher plains and hills, besides the increasing abundance of flowers and other seed-bearing plants and of the insects which visit them, existence for birds became possible, and with them that of a few scattered mammals of small size and generalized structure, with similar insectivorous habits.

During the age of reptiles, when they swarmed in every jungle, throughout the forests, and over the plains, competition rose so high that some of them were forced to take flight, and bat-like, provided with membranous wings, the pterodactyls lived in a medium before untried by any vertebrate, and finally there appeared in the Ornithostoma of the Cretaceous a colossal flying reptile, its wings spreading twice as much as any known bird, with a head four feet in length; its long toothless jaws closing on swarms of insects or perhaps small fry of its own type. But the experiment in point of numbers or capacity for extended flight did not succeed. Another type assayed the problem with better success. There appeared feathered and eventually toothless vertebrates with the fore extremities converted into pinions and the hinder ones retaining the raptorial reptilian form better adapted for aërial life. They eked out a by no means precarious existence on flying insects and seeds, as well as on the life in the soil, or by the seaside, and rapidly replaced certain older reptilian types. The class of birds has become about four times as numerous as the reptiles, and outnumbers the mammals nearly six times.

We may now review the zoölogical changes which took place at the time including the end of the Paleozoic and the opening of the Mesozoic. There was an extinction of the Tetracoralla and their replacement by corals with septa arranged in sixes; an extinction of cystidian and blastoid crinoids, the dying out of old-fashioned crinoids and echinoids (Palæocrinoidea and Palæechinoidea) fol-

lowed by the rise of their more modern specialized successors. As rapidly as the brachiopods became diminished in numbers, their place at the sea-bottom was taken by the more active and in some cases predatory bivalve and univalve molluscs. As the trilobites became extinct, their place in part was filled by their probable descendants, the Limuli, which had already begun to appear, the earliest types being Neolimulus, Exapinurus, and other forms of the Silurian, and Protolimulus of the Devonian. The Limuli of the Carboniferous, some with short (Prestwichia and Euproöps) and others with long tail-spines (Belinurus) suggest long possession of the soil and consequent variation and differentiation.

The Eurypterida shared the fate of the trilobites, and while there was a thorough weeding out of the more typical ganoids, leaving an impoverished assemblage to live on through after ages, that singular primitive vertebrate group, the Ostracodermi, was wholly obliterated.

On the other hand, with the incoming of a new order of vegetation, a great outgrowth of winged insects, the representatives of the orders of Lepidoptera and Hymenoptera, now so numerous in species, began their existence.

By the close of the Appalachian revolution, probably all the orders of insects had originated unless we except the most modified of all, the Diptera, whose remains have not been detected below the Lias. With but little doubt however, the eight orders of holometabolous insects diverged in the Permian, if not near the close of the Carboniferous, from some protoneuropter; the progress in the differentiation of genera and families becoming rapid either during the Jurassic or directly after the lower Cretaceous, or as soon as grasses and deciduous trees became in any way abundant.

Very soon, too, after the close of the revolution, the ancestral birds and mammals diverged from the reptiles, and of the latter the turtles, plesiosaurs, ichthyosaurs, crocodiles and dinosaurians, and soon after the pterodactyles, came into existence.

As a result of this revolution the molluscan type was profoundly effected, as at the opening of the Triassic siphoniate Pelecypoda, opisthobranchiate Gastropoda, and cuttles or belemnites appeared. While a few Orthoceratites lingered on after the revolution, the ammonites blossomed out in an astonishing variety of specific and generic forms.

In summing up the grand results of the Appalachian revolution

and of the times immediately succeeding, we should not lose sight of the fact that the changes in the earth's population were due not less to biological, than geological and topographical factors. The process of extinction was favored and hastened by the incoming of more specialized forms, many of them being carnivorous and destructive; as, for example, nearly all fishes and reptiles live on other animals. The struggle for existence between those which became inadapted and useless in the new order of things, went on more actively than at present. The process of extinction of the higher, more composite amphibians (the labyrinthodonts) was largely completed by the multitude of theromorphs and dinosaurs which overcame the colossal Cheirotherium, Mastodonsaurus and their allies.[1]

During the centuries of the Trias the lowlands became crowded, and the reptilian life was forced in some cases to gain a livelihood from the sea, for at this time was effected the change from small terrestrial reptiles like Nothosaurus to the colossal plesiosaurs and ichthyosaurs; in which digitate limbs were converted into paddles, and the ocean, before this time uninhabited by animals larger than ammonites, cuttles and sharks, began to swarm with colossal vertebrates, the increased volume of their new and untried habitat resulting in a tendency to a corresponding increase in weight, just as whales which possibly evolved from some land carnivore in the early tertiary waxed great in bulk, the increase in size perhaps having been due to the great volume of their habitat, the ocean.

Nothing so well illustrates the advantage to an incipient type as entering a previously uninhabited topographical area or a new medium, such as the air, in the case of the pterodactyles, the first vertebrates to solve the problem of aërial flight. Originating and prospering in the early Mesozoic, they held their own through the Cretaceous, where at their decline they became, as in Ornithostoma, colossal and toothless. We can imagine that the demise of this type was assisted in two ways: those with a feebler flight succumbed to the agile, tree climbing dinosaurs; while the avian type waxing stronger

[1] After writing the above lines I find the same view expressed in Woodworth's Base levelling and organic evolution. He remarks: "the exact cause of their decline is probably to be sought in the development of the more powerful reptilia" (p. 225). Regarding the circumstances favorable to reptilian life, he also states: "In the development of the peneplain from the high relief of the Permian and again at the close of the Jura-Trias, the widening out of the lowland, with plains and jungles, near tide-level, followed by depression of the land, must have highly favored the water-loving reptilia. It is to these geographical circumstances, I think, that we must look for our explanation of the remarkable history of this class in Mesozoic times" (p. 226).

in numbers and power of flight and exceeding in intelligence, exhausted the food supply of volant insects, and drove their clumsier reptilian cousins to the wall, fairly starving them out; just as at the present day the birds give the bats scarcely a *raison d'être*.

3. *The Pacific Coast Revolutions.*— It has long been known that there are a greater number of insect faunæ on the Pacific coast and greater variation of species, with more local varieties than east of the Mississippi river. It has also been shown by Gilbert and Evermann, as well as by Eigenmann, to apply to the fishes of the Columbia and Frazer river basins. "Nowhere else in North America," says the latter, "do we find, within a limited region, such extensive variations among fresh water fishes as on the Pacific slope." He also points out the noteworthy fact that the fauna is new as compared with the Atlantic slope fauna, and "has not yet reached a stage of stable equilibrium." As previously shown by Gilbert and Evermann, "each locality has a variety which, in the aggregate, is different from the variety of every other locality;" and he adds: "the climatic, altitudinal and geological differences in the different streams, and even in the length of the same stream, are very great on the Pacific slope."

It is evident that the variations are primarily due to the broken nature of the Pacific coast region, and to the isolation of the animals in distinct basins more or less surrounded by high mountain barriers, with different zones of temperature and varying degrees of humidity.

As brought out by the labors of LeConte, Diller, and Lindgren, the Sierra Nevada region has undergone cycles of denudation, and these changes, occurring later than those of the Appalachian region, have doubtless had much to do with the present diversified and variable fauna. The latest writer, N. F. Drake,[1] states that the western slope of the Sierra Nevada "was probably once a region worn down almost to base level or to a peneplain. By the uplift of the mountains a great fault was developed along the eastern face and the whole Sierra crust-block tilted to the westward. The streams quickened by the uplift again set to work on the peneplain and carried it to its present condition."

LeConte[2] states that the Sierra Nevada was upheaved at the end of the Jurassic period. This corresponded to the Appalachian revolution which occurred at the end of the Paleozoic era.

[1] The topography of California. Journ. of Geology, v., Sept.—Oct. 1897, p. 563–578.
[2] Bulletin Geol. Soc. Amer. ii, pp. 327–328.

"But during the long ages of the Cretaceous and Tertiary this range was cut down to very moderate height. . . . . The rivers by long work had finally reached their base-levels and rested. The scenery had assumed all the features of an old topography, with its gently flowing curves. . . . . At the end of the Tertiary came the great lava streams running down the river channels and displacing the rivers; the heaving up of the Sierra crust-block on its eastern side, forming the great fault-cliff there and transferring the crest to the extreme eastern margin; the great increase of the western slope and the consequent rejuvenescence of the vital energy of the rivers; the consequent down-cutting of these to form the present deep canyons and the resulting wild, almost savage, scenery of these mountains."

This view is further carried out by J. S. Diller, from his studies of the northern part of the Sierra Nevada, including the borders of the Sacramento valley and the Klamath mountains. He shows that northern California, during the earlier portion of the auriferous gravel period, was by long-continued degradation worn down to base-level conditions. "The mountain ranges," he says, "were low, and the scenery was everywhere characterized by gently flowing slopes." . . . .

"The topographic revolution consisted in the development out of such conditions of the conspicuous mountain ranges of to-day. The northern end of the Sierra Nevada has since been raised at least 4,000 feet, and possibly as much as 7,000 feet, and a fault of over 3,000 feet developed along the eastern face of that portion of the range "[1]

According to Lindgren the Sierra Nevada was eroded to, or almost to, a peneplain during Cretaceous times, and the mountains elevated in a later Cretaceous period were worn down during Tertiary times merely to a gentle topography.

The other post-cretaceous changes of this vast region are thus summarized by Scott from the results of Pacific coast geologists. In the Eocene a long narrow bay occupied the great valley of California extending northward into Oregon and Washington. At the end of the Eocene or early in the Miocene an elevation in California shifted the shore line far to the west. In the Miocene the Coast range formed a chain of reefs and islands, and at the close an upturning and elevation of the mountain range took place, though it became higher afterwards. The Coast range sank again early in the Pliocene and the San Francisco peninsula was an area

[1] 14th Ann. Rept. U. S. Geol. Survey. Part II, p. 433.

of subsidence and maximum deposition forming the thickest mass (58,000 feet) of Pliocene in North America. The mountains of British Columbia are believed to have been at a higher level than now, as it is supposed that Vancouver and Queen Charlotte Islands probably formed part of the mainland.

At or near the close of the Pliocene the Sierra Nevada increased in height by the tilting of the whole block westward. New river valleys, cut through the late basalt sheets of the Sierras, are much deeper than the older valleys excavated in Cretaceous and Tertiary times, owing to the greater height of the mountains and to the consequent greater fall of the streams. At this time the Wasatch mountains and high plateaus of Utah and Arizona were again upraised, and the great mountain barrier of the St. Elias in south-eastern Alaska was likewise thrown up. At this time also, or perhaps later, the mountains of British Columbia were probably raised still higher.[1] It will be seen from this that the present topography of the western border of our continent including Central America and the Isthmus of Panama belongs to a new topographic era, and fully substantiates the view that the fauna of these regions is very recent compared with that of the Atlantic border, and that the number of nascent or incipient species is much greater.

4. *The Upper Cretaceous Revolution.* — Another profound and epoch-making change occurred at the beginning of the Upper Cretaceous. In Eurasia, as Kayser states, "this was one of the greatest changes in the distribution of land and water over almost the whole earth, that is known in geographical history. Extensive areas which had for long periods been continents were now overflowed by the sea and covered with cretaceous deposits;" the upper Cretaceous strata in certain areas in Germany and Belgium resting directly on archean rocks. In America (the Dakota stage) there was also a great subsidence. The Atlantic coastal plain was submerged over what was Triassic soil, also the lowlands from New Jersey through Maryland to Florida, while the Gulf of Mexico extended northward and covered western Tennessee, Kentucky and Southern Illinois; a wide sea connected the Gulf of Mexico with the Arctic ocean, and thus the North America of that time was divided into a Pacific and an Atlantic land, the latter comprising the Precambrian and Paleozoic areas.

[1] Journal Geol. iv, pp. 882, 894, 897 and 898. (Quoted from Drake.)

As Scott states: "The Appalachian mountains, which had been subjected to the long continued denudation of Triassic, Jurassic, and Lower Cretaceous times, were now reduced nearly to base-level, the Kittatinny plain of geographers. The peneplain was low and flat, covering the whole Appalachian region, and the only high hills upon it were the mountains of Western North Carolina, then much lower than now. Across this low plain the Delaware, Susquehanna and Potomac must have held very much their present courses, meandering through alluvial flats" (p. 481). An elevatory movement began in the succeeding or Colorado epoch, and this was succeeded by an uplift on the Atlantic and Gulf coasts, and the continued upheaval in the interior resulted in the deposition of the Laramie brackish and fresh-water beds. There were similar widespread subsidences and upheavals in South America, the Andean chain being in large part upheaved at the close of the Cretaceous.

In the Cretaceous period there were such differences in the distribution of the fossils as to lead Römer, from his explorations in Texas as early as 1852, to consider that the resemblance of the fossils of Texas, Alabama and Mexico, with the West Indies and Columbia, to those of Southern Europe were due to differences of climate, a view reiterated by Kayser (p. 283). Scott also states that the Lower Cretaceous beds of Texas show faunal resemblances which ally them to the Portugal and Mediterranean beds, while the faunal relations of South American lower Cretaceous strata are closely like those of northern and western Africa.

The biological changes at the beginning of the Upper Cretaceous were correspondingly notable. Vast forests of conifers, palms, and especially of deciduous trees, such as the oak, sassafras, poplar, willow, maple, elm, beech, chestnut, and many others, clothed the uplands, while in the jungles, on the plains and in the openings of the forests gay flowers bloomed. The flora must even then have been, comparatively speaking, one of long existence, because highly differentiated composite plants, like the sunflower, occur in the upper Cretaceous or Raritan clays of the New Jersey coast. It may be imagined that with this great advance in the vegetables, the higher flower-visiting insects must have correspondingly multiplied in number and variety.

While the changes of level did not affect the abysses of the sea, the topography of the shallows and coast was materially modified,

and to this was perhaps largely due the extinction of the ammonites and their allies.[1] It is not impossible that the uncoiling of the ammonites into forms like Scaphites, Crioceras, Helioceras, Turrilites and Baculites, were originally perhaps distortions due to physical causes somewhat similar to those which produced a loosening or uncoiling of the spire in Planorbis. These variations or distortions of the pond snail, signs of weakness, the result either of pathological conditions or of senility, were due to unfavorable changes in the environment, such as either a freshening of the water or some other chemical alteration in the relative amount of alkalines and salts. The changes in the ammonites, though more remarkable, are similar to the aberrations observable in the shells of the upper and later layers of the Steinheim deposits, made known to us by Hilgendorf, Sandberger, and more especially by the detailed and masterly researches of Professor Hyatt.

In this case the Miocene tertiary *Planorbis lævis* was supposed to have been carried into a new lake, before untenanted by these shells. Although from some unknown cause the lake was unfavorable to the production of normal *lævis*, whose descendants show the results of accidents and disease, yet, owing to isolation, which prevented intercrossing with the present stock, and to the freedom from competition, the species was very prolific, and the lake became stocked with a multitude of more or less aberrant forms constituting new species. Some of them are nearly normal, with a flat spire, others are trochiform, and others entirely unwound or

[1] After preparing this address I find that Wood thirty-six years ago more fully discussed this matter and mentions the same cause I have suggested. "This disappearance," he says, "of the Ammonitidæ and preservation of the Nautilidæ, we may infer was due to the entire change which took place in the condition of the shores at the close of the cretaceous period; and this change was so complete, that such of the shore followers as were unable to adapt themselves to it succumbed, while the others that adapted themselves to the change altered their specific characters altogether. The Nautilidæ having come into existence long prior to the introduction of the Ammonidæ, and having also survived the destruction of the latter family, must have possessed in a remarkable degree a power of adapting themselves to altered conditions." On the other hand the dibranchiate cephalopods (cuttles or squids), living in deeper water, being "ocean-rangers," were quite independent of such geographical changes. Wood then goes on to say that the disappearance of the tetrabranchiate group affords a clew to that of the mesozoic saurians, and also of cestracionf sharks, whose food probably consisted mainly of the tetrabranchiate cephalopods. "Now the disappearance of the Tetrabranchiata, of the cestracionts and of the marine saurians, was contemporaneous; and we can hardly refuse to admit that such a triple destruction must have arisen either from some common cause or from these forms being successively dependent for existence upon each other." He also suggests that the development of the cuttles "has been commensurate with that of the cetacean order, of some of which they form the food. (Phil. Mag. xxiii, 1862, p. 384.)

corkscrew-shaped. Similar aberrations occur in *Planorbis complanatus*, living in certain ponds in Belgium (Magnon); in the slightly twisted Planorbid, *Helisoma plexata* Ingersoll of St. Mary's Lake, Antelope Park, Colorado, and in the unwound forms of Valvata first found by Hartt in Lawlor's Lake near St. John, New Brunswick, and described by Hyatt.[1] In all these cases of parallelism or convergence the aberrations seem to have been due to some unusual condition of the water adverse to normal growth. Hence it is not impossible that the singular uncoiled or straight forms assumed by certain of the Ammonites when on the verge of extinction, were likewise cases of convergence and due to weakness or senility, or at least to an unusual and unfavorable condition of the seas in which they lived.

The physical causes of extinction of the mesozoic reptiles may also have been due to or connected with the changes of coast level, although signs of weakness and senility are exhibited by these. In the Como or Atlantosaurus beds referred by Scott to the lower Cretaceous rather than Jurassic, the ichthyosaur (*Sauranodon natans*) was toothless, while the colossal Cretaceous pterodactyle, Ornithostoma (Pteranodon) was entirely lacking in teeth.

The colossal Pythonomorpha, offshoots of terrestrial lizards, but with paddles adapting them for marine existence, succeeded the plesiosaurs, and may have materially aided in their extinction. Hence arises the question, Did the extinction of the marine reptiles result in or contribute to the great increase of teleost fishes?

Before the dinosaurs began to die out the type in part became specialized into lizard-like tree-climbing forms, and agile bird-like forms. The first birds of the Cretaceous were toothed, carinate, highly predaceous forms, with a retrogressive side-branch of wingless diving birds, represented by the colossal Hesperornis, but in this case the loss of teeth was undoubtedly a gain to the type, compensation for the lack of a dental armature in the seed-eating birds being shown in the elaboration of a gizzard.

5. *Geological Changes in the Tertiary.*—Here again we have, as in former periods, a succession of earth-movements, subsidences in one region and elevations in another, though apparently more limited in extent than before, the oscillatory movements being rather confined to coastal areas, and involving adjacent shallow seas,

[1] Annual Report of Hayden's U. S. Geol. and Geogr. Survey Territories for 1874, Washington, 1876, p. 403.

there being frequent alternations of marine with brackish and fresh water beds. As Kayser remarks, the tertiary deposits "no longer extended unaltered over whole countries like those of older systems, but generally occupy only smaller basin or bay-like areas, filled up inland seas or shallow gulfs" (p. 328). Towards the close of the Tertiary the great mountain ranges of Asia and Europe, the Alps, Pyrenees, Caucasus, Himalayas, as well as the Atlas, and the Cordillera of North and South America were upheaved. The old Tertiary nummulitic beds were, in the western Alps, raised to a height of 11,000 feet and the Himalayas to a horizon 16,000 feet above the sea, while there were corresponding elevations in Western North America and in the Rocky Mountain region.

The evidence from fossils show, what has not been disputed, that climatic zones were by this time established. In Europe the older Tertiary was decidedly tropical, in the Miocene subtropical, but the climate of Europe was somewhat lowered late in the Miocene, as shown by the absence of palms.[1] At the end of the Tertiary *i. e.*, during the Pliocene the earth's climate was but slightly warmer than at present. It should be here noticed that while Greenland, Iceland, Spitzbergen, and Grinnell Land under 81° north latitude, were during the late Tertiary "abnormally warm," the tertiary floras of Northeastern Asia, including those of Kamtschatka, Amurland, and Saghalien, and that of Japan "show no sign of a similar

[1] Jaeger suggests that the occurrence in the later geological periods of warm-blooded vertebrates, protected by feathers or hair, was due to the fact that the earth then became cooler than in the preceding ages. His explanation of the origin of feathers and hair is as follows: "If the average temperature of an animal body is considerably higher than that of the surrounding media, oscillations of these media have a stimulating effect upon the skin of the animal. This leads to a tendency to form papillary chorian [*sic*] cells, and these afterwards produce hair or feathers, which represent two of the most characteristic features of warm-blooded animals." He adds that this "stimulatory effect upon the skin can only be due to low temperatures." The body temperature of the birds and mammals being high and the covering of the hair or feathers rendering them proof against the extremes of heat and cold, we can see that there is a coincidence between this and the fact that these classes began to increase in numbers towards the end of the Mesozoic, and especially at the opening of the Tertiary, when the climatic zones began to be established. So also in the case of whales, the loss of hair is compensated for by the blubber. Why, however, feathers developed in birds, rather than hair, is a problem no one has attempted to solve; though feathers of course better adapt the bird for flight; no flightless birds having such well developed feathers as those capable of extended flight. See G. Jaeger. Problems of Nature, Translated by Henry G. Schlichter, D. S. C., London, 1897. p. 66.

It might be suggested that the broad vane-like surface which characterizes feathers as compared with hairs may have been due to the fact that they would better support the body in flight; this difference from scales, as well as their greater lightness, giving this sort of armature an advantage over scales on the one hand and hairs on the other

warmth, but rather point to a climate colder than that of the present day" (Kayser, p. 354).[1]

The Tertiary was apparently also a time of more or less intercontinental migrations or interchange of life-forms, which crossed the oceans over so-called continental bridges. Bering Strait was at one time such a bridge, and to explain the geographical distribution of certain forms, there is thought to have been a more or less continuous land-connection between India and Africa, and between Africa and South America, and possibly in the Eocene, between Australia and southeastern Asia.

However hypothetical these continental bridges may be we do know that Central America and the Isthmus of Panama were elevated at the end of the Miocene, and that the bridge thus formed between North and South America became an avenue for the interchange of mammals and other animals which materially modified the distribution of life in the southern and northern parts of our continent.

The elevation of the West Indies took place at this date, and these islands were peopled from the South American coast. What we already know of the rapid evolution of molluscs, insects, and mammals on these islands shows how closely dependent variation and adaptation are on isolation as well as changed topographic and climatic features.

These problems have been studied with great care in the Hawaiian Islands by Gulick, and more recently by Hyatt. As well stated by Woodworth: "With the development of the umbrella-shaped topography of the Island of Oahu the land shells have varied from a common ancestral coastal type to valley-cradled, differentiated varieties, in the upper and disjointed valleys of this dismantled, volcanic island cone."[2]

The limits of this address do not permit us to treat at length of the wonderful changes, both geological and zoölogical, which occurred in western America during the Tertiary. They are now familiar to every one. The geological changes were very great and

[1] It has also been claimed by J. W. Gregory that the fossil plants of the Greenland miocene beds may have been drifted from the southward, and that the temperature of the polar region was not so elevated as Heer has led us to suppose. (Nature, vol. 56, p. 352, 1897.)

[2] The relation between base-leveling and organic evolution: referring to T. T. Gulick's article in Proc. Bost. Soc. Nat. Hist. xxiv, 1870, pp. 166–7.

widespread, as shown by the elevation of the land at the close of the Miocene. Fragments of the cretaceous sea-bottom, with horizontal strata, occur in the Rocky Mountains at a point about 10,000 feet above the sea. The inland cretaceous sea was drained off, and replaced by a series of fresh-water lakes, beginning with the Puerco, or the lowest Eocene and ending with the Pliocene lakes.

The most salient biological features of the Tertiary are the apparently sudden appearance all over the world of placental mammals, ending, if the deposits are truly pliocene, with the Java Pithecanthropus, and at the beginning of the Quaternary with paleolithic man.

The question here arises as to what retarded the progress in the mammalian types, although small, generalized, feeble insect-eaters had originated certainly in the Triassic and probably as early as the end of the Permian. We can only account for it by the unfavorable biological environment, by the apparently overwhelming numbers of mesozoic reptiles, adapted as they were for every variety of station and soil, whether on land, in the ocean, in the lakes and rivers, and even in the air.

When the reptiles became partly extinct, a great acceleration in the evolution of mammals at once resulted. There were now upland grassy plains, bordered by extensive forests, which also clothed the highlands, and all the geographical conditions so favorable to mammalian life became pronounced after the Cretaceous seas were drained off.

In his admirable essay on the relation between base-leveling and organic evolution, which we had not read until after planning and writing this address, though following the same line of thought, Mr. J. B. Woodworth suggests that mammalian life in the Mesozoic was unfavorably affected by the peneplain and by reptilian life.

"The weak marsupials or low mammals, which first appear in this country with Dromatherium in the tolerably high relief of the Trias, were apparently driven to the uplands by the more puissant and numerous reptilia of the peneplain. Their development seems also to have been retarded." Again he says: "To sum up the faunal history of the Mesozoic alone, we have seen that *pari passu* with the creation of broad lowlands there was brought on to the stage a remarkable production of reptiles, a characteristic lowland life; and we note that the humble mammalia were excluded from the peneplain or held back in their development, so far as we know them by actual remains, during this condition of affairs until

the very highest Cretaceous. At the close of the Mesozoic, the area of the peneplain was uplifted and there came into it the new life. Not only the changed geographic conditions, but the better fitted mammalia also were probably factors in terminating the life of the peneplains."[1]

After the placental mammals once became established, as the result of favorable geographical conditions, of migrations, isolation, and secondarily of competition, the evolution as well as the elimination of forms, as is well known, went on most rapidly. Remains of over two thousand species of extinct mammals during tertiary times which existed in America north of Mexico, have been already described where at present there are scarcely more than three hundred. This process of specialization involved not only the lengthening of the legs, the change from plantigrade to digitigrade, and to limbs adapted for seizing and handling their prey or food, or for swimming and climbing; the reduction of digits; the evolution of armatures, protective scales, etc.; but above all an increase in the mental capacity of the later forms, not only of mammals but of birds, as shown by the progressive increase in size of their brains; those of certain existing mammals being eight times as large, in proportion to the bulk of the body, as those of their early tertiary ancestors. This of course means that animal shrewdness, cunning and other intellectual qualities, the result of semi-social attrition and competition, had begun to displace the partly physical factors, and in the primates these may have in the beginning led to the appearance of man, a social animal, with the power of speech, and all the intelligent, moral and spiritual qualities, which perhaps primarily owe their genesis to increased brain-power.

The three most specialized types of mammals below man are the horse, the bats, and the whales. In the case of the bats, which appear in the Eocene, nature's experiment with these mammalian æronauts succeeded to the extent that they still exist in small numbers. Late in the Cretaceous or very early in the Eocene, competition apparently forced some unknown carnivorous type to take up an aquatic life, and the great success of the incoming cetacean type, resulting in the eocene zeuglodonts and miocene Squalodon, may have had an influence on the final extinction of the colossal marine reptiles.

6. *The Quaternary period.* — Coming now to the glacial epoch

[1] American Geologist, vol. xiv, Oct., 1894. pp. 209-235.

of the Quaternary period, we plainly see that under the extreme conditions to which life in the Northern hemisphere was exposed as never before, how intimate are the relations of geology and biology.

The rise of land at the beginning of the Quaternary, which carried the land and the life on it up into a cooler zone, with a mean temperature so low that the snows remained from century to century unmelted, forming continental glaciers, excited an immediate influence on the life. There were very soon developed a circumpolar flora and fauna, originating from the few Pliocene forms which became adapted to climatic conditions more extreme than ever before known in the world's history. While a few forms thus survived, some must have perished, though the bulk of them migrated southward.

The story told by the Port Kennedy hole, in Pennsylvania, just south of the limits of the ice sheet, is a most striking one. In that assemblage where are intermingled the bones of mammals of the Appalachian subprovince, with certain extinct forms, and those of the tapir and peccary and colossal sloths, adapted to the warmth of the pliocene and of the present Central American region we can realize as never before the immediate effect of a simple though very decided change of climate on organic life.

As a result of the submergence of the land in the north Atlantic and Arctic regions during the Leda or Champlain epoch succeeding, and the consequent amelioration of the climate, there was a return of a portion of the pliocene species to the vast area, thus freed from the presence of land ice.

Another effect of change of climate due to the further upheaval, drainage and drying up of lakes and river sources in the central portions of all the continents, was the destruction of forests resulting from the drying up of the lakes and streams, the formation of vast internal desert regions, with the desert floras and faunæ and saline animals peculiar to them; these are the last steps in geological history of the origination of species, and have been taken almost under the observation of man. In the origin of species adapted to desert areas and to salt lakes, *faunæ relictæ* of the lakes on the elevated plains of Asia, South America, Africa, Sweden and the Great Lake region, we see that geographical isolation and the absence of competition are the primary factors in the case.

In conclusion, it is, from the nature of the case, notwithstanding the imperfection of the geological record, apparent that the fullest, most complete and convincing proof of organic evolution is derived from the past history of life, from paleontology, which involves the fact of geological succession. Looking back for half a century we see that organic evolution is a fact, and is grounded and dependent on geological evolution, and the latter on cosmical evolution. Should we ever have to abandon the principle of evolution we should also have to give up the theory of gravitation, the principle of the correlation of physical forces, and also the conception of the unity of nature. All of these principles are interdependent, and form the foundation stones of our modern science.

The rapid summary we have given of the successive changes and revolutions in the earth's history and the fact that they are accompanied or followed by the process of the extinction of the unadapted, and their replacement by the more specialized and better adapted, show that there is between these two sets of phenomena a relation of cause and effect.

Moreover it cannot be denied that the formation of our solar system in the manner outlined by the founders of the nebular hypothesis, that the progressive changes in geology and the earth's topography, the gradual building up or evolution of the continents, and the increasing fitness and intelligence of the life on its surface, the final outcome being man, whose physical development was practically completed at the beginning of the quaternary period, and whose intellectual and moral improvement have as it were but just begun — the scientist, as such, can scarcely deny that this process of evolution, along so many lines and involving not only material but mental and moral advances, has gone on in an orderly and progressive way. The impression left on the mind is that all these changes, inorganic and organic, have been purposive rather than fortuitous, the result of the action of natural laws, impressed on matter by an Intelligence and force outside of but yet immanent in all things material.

With Hutton we may say: "We have now got to the end of our reasoning; we have no data further to conclude immediately from that which actually is. But we have got enough,—we have the satisfaction to find that, in Nature, there is wisdom, system and consistency."

Here, as men of science merely, we may pause and confess our

ignorance of the first or ultimate cause of this progressive evolutionary movement pervading the material universe and, suspending our judgment, assume an agnostic position. But the human mind, even when rigidly scientific and logical, is so constituted that few of us are satisfied to stop here. He who is most capable of daring speculation in the realm of physical or biological or philosophical thought cannot refrain from inquiring into the nature of the First or moving cause, and how the present order of things has been brought about.

As a mere working hypothesis, we are, at least many of us, compelled to assume that the present order of things, material and immaterial, is not self-evolved, but is the result of an Infinite Intelligence and Will giving the initial impulse, and dominating as well as guiding and coördinating the progressive changes, whether cosmical, geological or biological. The fact of the survival of the fittest, of the extinction of the unfit, the conclusion that throughout the universe order has arisen from chaos or the undifferentiated, the specialized from the generalized, that the good, the beautiful, and the true have in the past overcome and will continue to outweigh what is unfit and evil in matter, mind and morals, at least strongly suggests that the First Cause is not only omnipotent but all-wise and beneficent. For evolution tends to optimism. Few working biologists are pessimistic. And thus while science as such is concerned with facts and their relations, we can at the end of this century of scientific effort, affirm that it need not be and is not opposed to whatever is noble, exalted, hopeful, and inspiring in human aspirations, or to the yearnings of the soul for a life beyond the present, for there certainly are in the facts of the moral and spiritual evolution of our race intimations of immortality, and suggestions, where absolute proof is naturally wanting, of a divinity that shapes the course of nature.

PAPERS READ.

[ABSTRACTS.]

EVOLUTION AND MIGRATION OF HAWAIIAN LAND SHELLS. By Prof. ALPHEUS HYATT, Boston Society of Natural History, Boston, Mass.

AN investigation of the Achatinellidæ was begun by the author some seven or eight years since in consequence of the purchase of a collection of these shells from Rev. J. T. Gulick. On account of the accuracy of the labeling of this and other collections more recently acquired, among which was Gulick's personal collection of about 10,000 shells and Oleson's loan of about 6,000 shells, it has been possible to arrive at certain definite conclusions in regard to the migration and evolution of the three principal genera of this family upon the island of Oahu. This island contains many more species than all the other islands together; estimating the other Islands to have about 140 species, Oahu has about 280.

The author has had the use of about 22,000 shells, of which 18,000, at least, were collected in Oahu. The three genera, Bulimella, Achatinella and Apex, were plotted upon a relief map of Oahu by means of pins with different colored heads. Each pin represents the occurrence of a species in the valley in which it is placed and each species has its own special number, which with the specific name is included in labels on the pins that represent the shells on the map. The red pins represent species of Achatinella, the blue those of Apex, and the white those of Bulimella. Lines of colored worsted connecting the pins indicate genetic affinity and the supposed descent of the species one from another, as the shells wandered northwards from the southernmost valley, Kuliouou.

All of the shells probably sprang from a common ancestor which has become extinct of late years; but was formerly collected in a valley on the western side of the watershed of the longest or easterly range. From this valley, Kuliouou, there was a migration northwards across the spurs. The ancestral species, *Achatinella phæozona*, Gulick, as it marched to the north evolved a number of genera, of which the three principal ones are represented on the map, and expanded also into a large number of distinct species.

The Bulimellæ live habitually highest upon the sides of the mountains. These alone cross the great divide to the eastward and are successful in evolving considerable numbers of large shells of different species on that side of the watershed. This eastern side is exposed for nine months in the year to heavy winds, the trade winds, and has a rougher climate and

less vegetation than the inner or more sheltered land side. This genus however does not succeed in forming colonies in the short mountain range which occurs on the western side of the island.

The Achatinellæ live in the valleys lower down than the Bulimellæ; they cross to the eastern side in several places, but all of these species lose in size and do not evolve any great number of forms or varieties and show that they are unable to contend successfully with the climate on that side. The species of this genus also become smaller and less brilliant in color in the northern valleys of the western, or inner, watershed, of the eastern range, the same holding true in those that occupy the outer, or eastern watershed of the same range. They were unable to cross the lowlands in the center of the island, except in such sporadic examples that the inference is that they were unable to reach and colonize the western range.

The species of Apex live in the valleys mixed with both the other genera but do not habitually reach so high as the Bulimellæ nor to as low a level as the Achatinellæ; they seem to have been even more delicate than those of Achatinella and less able to contend with the unfavorable effects of the surroundings on the ocean side of these hills, since not a single species has been recorded as found on that side. On the other hand they cross the broad plain of the interior, and are successful on the short western range, where only a few sporadic species of the other two genera are found.

VARIATION IN THE SHELL OF HELIX NEMORALIS IN THE LEXINGTON, VA., COLONY. By Prof. JAS. LEWIS HOWE, Washington and Lee University, Lexington, Va.

THREE series of over 1,000 shells each, were collected, one in the author's garden, in 1897, one in same garden, in 1898, and the third in 1898, in the garden where the colony originated in 1883. From a study of these series the conclusions are drawn: (1) that the variations tend in general along the same lines as the variations in Europe; (2) the tendency of variation is different in different portions of the Lexington colony; (3) no influence was noticed in a given locality by a very considerable destruction of individuals. The author gave a list of the varieties known in the Lexington colony. The total number of varieties is 385 of which 277 are enumerated for the first time.

[This paper is printed in full in American Naturalist.]

VARIATION VERSUS HEREDITY. By Prof. H. S. WILLIAMS, New Haven, Conn.

THE author considers that *variation* and not *heredity* is the fundamental characteristic of the phenomena of organisms. Heredity is of the nature of habit, or memory, and implies experience. Variation is, in its intrinsic nature, original and genetic. So long as the varying does not

exceed the varying of previous organisms, the process is called individual development and is purely hereditary. Whenever the varying results in producing a structure not hitherto produced it is evolution. The most successful is the one which gets the most result out of the resources at hand, and such success survives. But this is the law of variation, not of heredity. The arguments for variation *versus* heredity are as follows:

1. In any concrete case of natural selection, or similar processes, the actual result of selection is the retarding and checking of variation, and the offspring necessarily evolves more slowly than its parent in direct proportion to the efficacy of the natural selection.

2. That the organic processes, by which variation takes place in an organism, differ from the ordinary process of development in individual growth, only by passing beyond the limit reached by the ancestor; and hence variation is but a phase of the fundamental genetic process peculiar to living organisms.

3. That every act of variation is anterior to experience, and thus is necessarily original and genetic; whereas every hereditary act is necessarily secondary to, and the result of experience, and the law of heredity must, therefore, be acquired in the process of evolution and is not fundamental.

4. That as to struggle for existence, the most strenuous effort made (both by the parent and by the offspring) in the course of organic processes, is that which produces antagonism of interests. On the part of the parent, it parts with that which has cost it the greatest expenditure of energy,—and on the part of the offspring, the result is the loss, in part or wholly, of the only source of its living up to the moment of the struggle.

5. That the orthodox view of the case is inconsistent in so far as it recognizes mutability as applicable only to organic species and clings to the idea of immutability of the more fundamental units of biology,—viz., the individual and the cell and the protoplasmic states of matter.

These considerations bring us to a point of view in which heredity and variation hold a different relation to evolution than in the ordinary working hypothesis of biology.

If this point of view presents the facts in their true relations, we must seek for the immediate determining causes of variation, not in natural selection, nor in any of the environmental conditions, either direct or indirect, by which hereditary repetition is established, but in the phenomena of individual growth and development, and the more fundamental processes of cell growth and metabolism.

[To be published in the American Naturalist.]

LOCALIZED STAGES IN GROWTH. By Dr. ROBERT TRACY JACKSON, Harvard University, Cambridge, Mass.

IN the development of animals, stages are found which repeat the form of adults of ancestral types. In old age, senile stages occur which are

prophetic of degradational series of the group. The ontogeny of an individual represents therefore in an epitomized form the phylogeny of the group. Stages then, are looked for in the young and in the senile individual, the adult representing the full specific characters.

From studies of animals and plants it has been found that stages may occur in localized parts throughout the life of the individual. In organisms, that during growth present a serial repetition of parts, there is often an ontogenesis of such parts which is more or less closely parallel to the ontogenesis of the organism as a whole, and repeats the characters seen in similar parts in the young individual, and in the adults of ancestral types. The characters repeated are usually post-embryonic, or nepionic characters.

In the asexual reproduction of Hydrozoa and Actinozoa, buds are given off, which during growth repeat the characters seen in later stages of embryos from the egg. The young plates of the stem of certain crinoids throughout life repeat the features of the whole stem of ancestral forms. The young last-formed plates of Lepidechinus are not specialized like the older plates, but resemble those of more generalized types. In the development of Ammonites the first septa are simple, the later ones more or less complex. The portion of the complex septum which lies close to the umbilicus is relatively simple, in this localized area repeating youthful and ancestral characters.

In plants, suckers from the roots, or base of the stem, or stump, often repeat the characters of seedlings. This is seen in the Pitch Pine, Areca, Oaks, White Ash, Sassafras, Negundo, Ailanthus. In the Japanese Ivy (*Ampelopsis tricuspidata*), at the base of the vine throughout life, are compound leaves like those of the seedling. On all the old wood leaves are simple and trilobed. On the new wood of the current season's growth, the leaves are cordate like a late stage in growth of the seedling. Feeble degradational growths may be reversionary, thus presenting localized stages, as the tuftlike growths of Red Cedar, which bear acicular leaves like the young and fossil species. The terminal portion of the leaf often repeats the simple characters of the leaves of the seedling in a very close degree. This is seen in the Tulip tree, Ashes, Ailanthus, Phœnix. Reversionary individual leaves often occur on adult plants. These have failed to develop the full specific characters, bear more or less resemblance to the young and fossil allies and are considered localized stages in growth. Such are seen in the Tulip tree, Ashes (especially *Fraxinus anomola*), Negundo, Palms.

ON THE CARBONIFEROUS FAUNA OF RHODE ISLAND AND MASSACHUSETTS.

By Prof. A. S. PACKARD, Brown University, Providence, R. I.

THE fossil animals in the Narragansett coal basin are very scanty. The fauna so far as yet known comprises Spirorbis, worm holes, and the doubtful impressions of two Annelids, a bivalve, *Anthracomya arenacea* (Dawson), tracks supposed to be those of an arthropod, probably the

same as *Protichnites carbonarius* Dawson; an arachnid and fourteen species of insects, mostly cockroaches.

The presence of the Anthracomya and Protichnites, which have recently been detected, leads us to refer the beds to the same horizon as the South Joggins beds in Nova Scotia, which are Upper Carboniferous. Thus far the animal remains confirm Lesquereux's reference of the plant beds of Rhode Island to the Upper Carboniferous.

ON THE PRESENT STATE OF OUR KNOWLEDGE OF THE NORTH AMERICAN TERTIARY MOLLUSK-FAUNA. By Prof. WM. H. DALL, Smithsonian Institution, Washington, D. C.

THE author briefly reviewed the successive fields covered by the earlier American workers, beginning with Say and Lesueur, in 1822, and coming down to 1860, forming the first period of American study in this direction, chiefly on the horizons of the coastal plain of the southeastern United States. Sowerby, Guppy and Gabb had busied themselves chiefly with the Oligocene and Pliocene beds of the Antillean region.

The second period began with the organization of the U. S. Geological Survey about 1880, when a renewed interest in the subject sprang up, and by 1885. new publications began to be fairly numerous. The characteristics of the new work were the revision of the older faunal lists, the discrimination of horizons, often neglected by the earlier writers, the correlation of continental and Antillean horizons and the discovery and exploitation of horizons previously unknown. The most important of these were: the Pliocene of Florida, which has been explored by Willcox, Heilprin and Dall; the Upper Miocene of the Galveston, Texas, artesian well from which a distinctive fauna was obtained by the Texas Geological Survey and worked up by G. D. Harris; and the upper Oligocene of western Florida where a whole series of beds has been discriminated by Langdon, Burns, Aldrich, Dall and others, constituting a series of at least three distinct horizons, each with a rich fauna, and which has furnished to our catalogues a multitude of forms previously unknown or not known to occur in North America.

LEIDY'S GENUS OURAMŒBA. By WM. L. POTEAT, Wake Forest, N. C.

THE history of the genus is brief. The first published notice was by Mr. Archer in 1866,[1] and again in 1873.[2] The next year Dr. Joseph Leidy[3] described the same or a very similar organism. In 1875 he published

[1] Quart. Journ. Micr. Soc. 6, 190.
[2] *Idem*, 14, 212.
[3] Proc. Acad. Nat. Sci. Philadelphia, 1874, p. 77.

a more detailed account,[1] and gave his final treatment of it in Freshwater Rhizopods of North America, 1879. In 1884 Gruber[2] treated as fungoid the caudal filaments on his *A. binucleata* and other forms. My studies of Ouramœba began in February, 1894. I published in Nature[3] my conviction that Leidy's view of the filaments as extensions of the body protoplasm was erroneous, and that they were mycelial filaments of a fungus parasitic upon the genus Amœba. Early in 1896 Mr. Martin F. Woodward, of the Royal College of London, wrote me of his agreement with my interpretation of the peculiar structure, and later, with great generosity, put notes of his observations and drawings at my disposal.

THESIS: Leidy's genus Ouramœba was erected upon insufficient data and must be abandoned. The filamentous appendages, which constitute the only peculiarity of the form, are not, as he and Archer supposed, extensions of the body substance, but are mycelial hyphæ, commonly unseptate, which spring from a spore lying in the endosarc, and represent a semi-parasitic relation to the genus Amœba. The following facts are submitted in support of this view.

First, *morphological* facts. Mention is made only of what Dr. Leidy omitted to notice or failed to see the exact signification. (1) The single or tufted filaments invariably arise from a spore. The fact that the mycelium resulting from the germination of the spore, instead of ramifying through the protoplasmic mass, issues at once into the water, suggests that the fungus does not subsist at the expense of the Amœba, but upon organic substances dissolved in the water. Some of these are, doubtless, contributed by the contractile vacuole, which lies in the neighborhood of the filaments and discharges its contents among them. (2) The structure of the filaments is that of unseptate hyphæ, not of pseudopodia. An occasional septum at the origin of a branch makes the pseudopod interpretation impossible. (3) After separation from the Amœba the filaments maintain in all respects their characteristic structure and relations.

Second, *physiological* facts. (1) The form on which Messrs. Archer and Woodward found in the British Isles the fascicled hyphæ was *Amœba villosa*, while the host form in this country is *Amœba proteus*. (2) The seasonal limitation of the occurrence of the form, it being confined apparently to the months January to May. (3) The hyphæ take no part in any of the movements of the host, being altogether passive and non-retractile. (4) The progressive development of a single tuft is sufficient in itself to establish the main point of my thesis. The inquiry into the life history and relationships of the fungus itself is not entered upon here.

MONTICULIPORIDÆ, A NEW FAMILY OF PALÆOZOIC CORALS. By AMADEUS W. GRABAU, Cambridge, Mass.

[1] *Ibidem*, 1878, p. 128.
[2] Zeit. f. Wis. Zool., 41: 210
[3] Nature, 50: 79.

NOTES ON ÆOLOSOMA TENEBRARUM. By Miss EDITH M. BRACE, Rochester, N. Y.

Æolosoma tenebrarum is a small oligochaete common among algæ in stagnant ponds. It has held an anomalous position among annelids from the fact that the nervous system has been supposed to consist of a brain, without a ventral cord or œsophageal commissure, but new methods have made it possible to demonstrate that the nervous system conforms with the annelid type.

The nervous system is connected throughout with the epidermis The brain, which lies in front of the mouth, has two lateral lobes running to the œsophageal commissure, and two posterior lobes composed entirely of cells. There is a pair of ganglia on the œsophageal commissure just before its union with the brain, and a second pair below the mouth and in front of the first segment. A pair of nerves extends over the prostomium from the first pair of ganglia, a pair from the anterior margin of the brain, and a third pair runs back from the angle between the lateral and posterior lobes of the brain. Two rows of bilobed ganglia connected by fibrous commissures form the ladder type of ventral cord. There is one pair of ganglia in a segment, each ganglion has four lateral nerves, and there is a large median nerve.

A diffuse plexus of nervous tissue extends through the epidermis and brain. It consists of multipolar cells which send out anastomosing branches and form a coarse network. The system has an oral ring sending branches to plexus cells in the brain from which branches run to a crescent-shaped band near the anterior margin of the prostomium. Two club-shaped bodies in connection with this centre may serve as olfactory organs.

The sympathetic system consists of an arborescent network extending the length of the alimentary tract. It has an oral ring between the œsophageal commissure and the oral ring of the epidermal plexus. The ciliated pits described by Vejdowsky prove to be terminations of ciliated furrows leading to the mouth. Sense organs: There are many large pear-shaped cells which contain a refractive body. Also compound bodies similar to the eyes found in other worms. These organs are innervated by the epidermal plexus.

A NEW CLASSIFICATION OF FOSSIL CEPHALOPODS. By Prof. ALPHEUS HYATT, Boston Society Natural History, Boston, Mass.

THE Cephalopoda with camerate shells embrace two orders, Nautiloidea and Ammonoidea, since the shells of Belemnoidea are very properly regarded as internal structures. The classification of Nautiloidea is in confusion, owing to the overthrow of the old systems, which assembled the groups according to their forms into straight shells or Orthoceratites, covered shells or Cyrtoceratites, loosely coiled shells or Gyroceratites and

closer coiled shells or Nautilites. In consequence of this and the increase in our knowledge of forms, it is high time that some attempt should be made to solve the question of the true relations of the larger groups, including the families and genera. This attempt has been made by the author in preparing the chapter on Fossil Cephalopoda in the English edition of Zittel's Textbook of Paleontology which is now in press.

After reviewing all the genera and a considerable proportion of the species, it was found that the only characters having sufficiently broad distribution to be used for the diagnosis of suborders were those existing in the structure of the siphuncles. The siphuncle was of much greater importance in ancient times than later. In primitive forms as compared with more specialized forms the same law holds and this is equally true of the young as compared with the later stages in the ontogeny. This assumption is based upon the larger proportionate size and more or less complicated internal structures of this organ in the more ancient and more primitive forms as well as its greater size in the young of all shells. It is natural, therefore, that the changes in the structure of this organ should prove to be of use in classifying the suborders and often families as well. In this paper I propose merely to notice the suborders which are as follows:

I. Holochoanites with the funnels of the siphuncle reaching entirely across each air chamber completely shutting off the interior of each chamber from the interior of the siphuncle. Whenever deposits are present they are more or less prolonged cones or irregular structural filling around the endosiphuncles that are often present and pierce the deposits at the center. Examples: Endoceras, Piloceras, Nanno.

II. Mixo-choanites. Siphuncles small. Funnels composite. They are short and straight in the young, and the closing of the siphuncles was made by sheaths reaching from the ends of the funnels to the next septum. In the ephebic or later stages, collars were added, making the funnels diplo-choanitic. In old age they were apt to become more or less beaded or imperfect. No internal deposits. Examples: Ascoceras, Aphragmites.

III. Schiztochoanites. Siphuncles large, funnels imperfect, or split on the outer side. These were at the bases of flat rings or collars that extend into the interior, but are open at the center. This has been simultaneously and independently worked out, by Professor Dwight of Vassar College and the author, in different ways. Dwight by means of sections, and the author by the use of fractured specimens. Examples: Conoceras Bronn (Bathmoceras Barrande).

IV. Orthochoanites. The siphuncles small except in primitive forms and without deposits, or, if present, these are irregular and are gathered around the funnels. No endosiphuncles. The funnels are straight and the siphuncle is apt to be tubular. Examples: Orthoceras, Geisonoceras. It is very difficult to separate some forms of this suborder from the Annulosiphonata, although a large proportion can be diagnosed without difficulty.

V. Cyrtochoanites. The siphuncles are of different sizes, but the funnels are invariably short and much bent outwardly. There are two subgroups in this suborder.

(*a*) Annulosiphonata. Siphuncles large in primitive forms only, and the segments beaded. Internal deposits are gathered about the funnels and when abundant enough to fill they meet along the median horizontal planes, bisecting the segments and their intersections, therefore, alternate with the septa. There are also invariably concentric layers advancing towards the interior as their thickness increases by accretion of new layers A large endosiphuncle is often present in the primitive forms. Examples: Actinoceras, Loxoceras.

(*b*) Actinosiphonata. The internal deposits are arranged as vertical, radiating plates directed towards the interior, but not meeting, and the center consequently must have been occupied by a peculiar endosiphuncle. These deposits increase in number of plates and also by accretion at both ends, so that they often meet those of the next segments. The lines or planes of junction coincide with the funnels and septa, being in the same horizontal planes with the latter. Examples: Jovellania, Rizoceras.

ON THE SYSTEMATIC POSITION OF THE TRILOBITES. By Prof. A. S. PACKARD, Brown University, Providence, R. I.

THE reference by several writers of the Trilobita to the class of Crustacea seems still open to question.

The recent discoveries of Walcott, Beecher and others, as to the nature of the appendages, especially those of the head, afford data for the further discussion of the question.

Beecher has clearly shown that, while Triarthrus has antenna-like appendages, all the segments behind the antennal, bear biramose limbs, which are essentially the same for the head and trunk. In Crustacea, on the other hand, even in the most generalized, there is a differentiation of the appendages into two pairs of antennæ, mandibles, maxillæ, maxillipedes, thoracic and abdominal limbs.

For this reason, together with the different larval forms, and from the obvious affinities of trilobites to Limulus, which certainly is not a crustacean, we are inclined to refer the trilobites to a separate class, and to regard them as being an older, more primitive group than the Crustacea. It also seems more probable that the merostomes, including Limulus, descended from the trilobites, or forms like them. Hence the Trilobita probably were the first to originate from the annelid worms, while the Crustacea arose by a separate line of descent.

MEASUREMENTS OF TWO LARGE LOBSTERS, RECENTLY ADDED TO THE COLLECTIONS OF THE AMERICAN MUSEUM OF NATURAL HISTORY. By Dr. E. O. HOVEY, American Museum of Natural History, New York, N. Y.

TWO unusually large lobsters were caught off Atlantic Highlands, N. J., in the early spring of 1897, one of which was reported to weigh thirty-

one and the other thirty-four pounds. They refused to live in captivity and their carcasses were presented to the museum for preservation. As mounted, these are about thirty-seven and forty inches in length. The paper gave a series of measurements made on the mounted specimens.

A NEW METHOD OF STUDYING UNDERGROUND INSECTS. By Prof. JOHN B. SMITH, Rutgers College, New Brunswick, N. J.

THE methods heretofore used to study the habits of underground insects have left much to be desired. Digging down so as to follow the course of a burrow is extremely unsatisfactory, and in many cases impossible. Experiments were made with various materials, with the idea of hardening the earth around the channels made by the insects, so that they could be dug out in the shape of pipes; but this proved unsatisfactory with all the materials used.

At the suggestion of Mr. J. Turner Brakeley, of Bordentown, N. J., plaster of Paris was tried and a series of experiments were made by him at my request. It was proved that plaster of Paris, sufficiently thinned down, would serve to make casts of insect burrows extending nearly six feet underground and branching in various directions. As much as nine ounces of mixed plaster has been poured into an opening not more than one-fourth of an inch in diameter, and a perfect casting of all the underground workings of the insect has been obtained. Casts have been made of burrows not more than one-eighth of an inch in diameter, and extending down a distance of eighteen inches. In some cases, where an attempt has been made to obtain casts of the workings of ants, burrows not more than one-sixteenth of an inch in diameter have been cast; but in such cases large chambers, opening out from such small galleries, will not become filled with the plaster, because this seems to set in the smaller burrows before the larger chambers are filled.

Just what the range of usefulness of this plaster method will be, cannot yet be positively stated. It will prove satisfactory, however, for almost all of the burrowing bees and wasps, for spider architecture, and to obtain casts of the burrows made by many of the coleopterous larvæ that live underground.

There is considerable difference in the character of the plaster and the better the plaster the better the resulting cast. Experience has proved that dental plaster of the best quality, mixed with an equal bulk of water, gives the best results. The plaster used by me was obtained from H. D. Justi & Son, of Philadelphia. It sets rapidly and makes a very hard cast. The mechanical method is simply to have two graduates; in one of them measure the water and in the other an equal bulk of the plaster. The graduate containing the water should be sufficiently large to allow the plaster to be dumped into the water. The addition of the plaster will add about one-fourth to the bulk of the mixture; that is, four ounces of plaster and four ounces of water will make five ounces of mixture. Stir thoroughly and pour at once.

NOTES ON THE HABITS OF SOME BURROWING BEES. By Prof. JOHN B. SMITH, Rutgers College, New Brunswick, N. J.

BY means of the plaster method referred to in a previous communication the habits of several species of burrowing bees were studied in Ocean County, N. J. The species involved are *Colletes compactes*, *Andrena vicina*, *A. bicolor* and *A. viola;* also *Augochlora humeralis*.

The habits of the Andrenids are very similar. *Colletes* appears very early in spring, during the first warm days of March or April, and at once digs down, almost vertically, a distance of some eighteen inches. It may go deeper, and one specimen was found that burrowed down twenty-seven inches almost vertically. Then a horizontal burrow is sent off to a distance varying from two to five inches and at the end of this burrow a very thin, parchment-like cell is constructed. The cell seems to be made of saliva which dries and forms a brittle chamber in which the pollen and honey are stored. Not more than one-third or even less of the cell is filled with pollen and honey.

The egg is attached at about the middle of the cell at one side and curves so that its tip reaches nearly, if not quite, to the surface of the pollen mass. Just enough honey is added to the pollen to make a stiff paste and upon this the larva feeds. When one cell is completed, it is closed by a thin membrane, such as that of which the balance of the cell is made and then the insect fills the burrow, or it collapses and the insect starts a new lateral at some distance above the first. It may be that the earth taken out of the new lateral is used to fill up the old one. Casts, made after the first lateral has been filled, show a heel, which extends a short distance below the entrance to the second or upper lateral. At the end of the second lateral, which usually goes off in a different direction from the first, is placed a similar cell. A third lateral may be built above the second, which brings the insect usually between six and twelve inches below the surface. In no case have I found more than three laterals from a single main burrow.

When the insect has charged all the laterals the entire burrow is filled up, so that the young bees when they come to maturity must bore through from six to twenty-four inches of sand or earth before reaching the surface. Cells from which the bees have emerged are always found entirely filled with sand or earth; that is, as the bee moves out of its cell, it fills it with the earth which it excavates before it, and in this way works upward, always throwing the earth behind into the spaces that had been excavated just before.

Andrena bicolor and *vicina* make burrows that are considerably larger in diameter and are very much twisted instead of going down straight. They make a number of twists and turns and may extend forty inches or more below the surface. One burrow extending forty-eight inches has been cast and taken out. Their general habits of breeding are very much like those of *Colletes;* but they do not begin to dig until May.

Some of the smaller species of Andrena work on a somewhat different plan, and line the inside of their cells with a sticky fluid that holds the earth together. In such cases the cell appears black inside, and the pollen mass is formed in the shape of a loaf, upon which the egg is laid. Very little honey is used by these bees. So far as their habits have been studied they seem to be much like those of the previous species.

Augochlora works on a different plan. It makes a perfectly vertical burrow which may extend down nearly six feet underground. At a distance of about twelve inches it sends off a short lateral, then excavates a chamber sufficiently high to allow it to move about in every direction and this may be anywhere from one to two or even two and one-half inches across. Cells are then sent down from this chamber, usually beginning at the middle, and there may be a cluster of anywhere from six to twenty of them entered from the chamber above. The cells are lined with clay so as to make them tight, and as soon as one is completed, this being generally the one in the center, the insect places it in a pile of pollen formed into a loaf; lays an egg on the pollen and closes the cell with clay. Two or even three such clusters of cells may be made by a single bee and thirty or forty cells may be found in connection with a single cast.

A peculiar feature in the habit of the insect is that when it starts its burrow, somewhere during the latter part of May, it makes first an oblique gallery from four to six inches in length before it starts straight down. All the earth is brought out through this oblique gallery. After it begins to bore vertically it extends the upward burrow to just below the surface and even makes a small opening to it which is usually concealed and where no sand is ever piled. This opening is the one that is used by the bee in leaving and entering its home. Whenever it is in the burrow the entrance is plugged up by a ball of yellow sand or clay. This is kept intact so long as the bee works, and it may work for several days underground without coming out to feed.

These bees do not fill their burrows after they have filled the cells. The young when they make their exit come through the parent's burrows and these parent's burrows are used by the insects as hibernating quarters. Early in spring the female bees can be found at the bottom of the vertical burrow and as many as eight have been found piled one on top of the other, five feet underground.

ON THE MARKINGS OF NOTODONTIAN LARVÆ. By Prof. A. S. PACKARD, Brown University, Providence, R. I.

IN his studies on the markings of lizards, Eimer has shown that the changes from stripes to spots originate near the tail, and extend forward in waves, so to speak. Weismann has shown that the same happens in caterpillars. Cope noticed the same phenomenon in the lizard Cnemidophorus, in which a longitudinally striped coloration changes into one trans-

versely striped, the breaking up of the striped coloration appearing first at the posterior part of the dorsal region of the body.

We have observed the same thing in several notodontian larvæ, *i. e.*, *Symmeristia albifrons*, *Dasylophia anguina*, and *Schizura concinna*. In these forms the longitudinal bands become broken up into spots at the end of the body, on the last four or last three segments, where the banded arrangement disappears.

The cause of the origin of the change in markings at the end of the body in lizards is not easy to explain, but in caterpillars it may be connected with the fact that in young arthropods the new segments originate at the "budding zone," or the growing point, *i. e.*, the region between the last body-segment and the penultimate segment.

THE PROPOSED ATTEMPT TO INTRODUCE BLASTOPHAGA PSENES INTO CALIFORNIA. By Dr. L. O. HOWARD, U. S. Department of Agriculture, Washington, D. C.

THE RECORDS FOR 1898 OF BROODS VII AND XVII, OF CICADA SEPTENDECIM. By C. L. MARLATT, U. S. Department of Agriculture, Washington, D. C.

NOTES ON THE LIFE HISTORY OF PROTOPARCE CAROLINA. By Prof. WM. B. ALWOOD, Va. Polytechnic Institute.

THE breeding work conducted with this species covering a period of two years showed that it is to a slight extent double-brooded, at Blacksburg, Va. The earliest adult moths few in number appear June 7-12, oviposition begins June 20th, the larvæ moult four times, at intervals almost invariably of four days each, become full fed in twenty to twenty-one days and enter the soil for pupation. A small per cent of the early brood issue as adults the same season, the records showing a period of twenty-seven days from pupation to issuance of imago in one case, the shortest observed.

The greater part of the moths are single-brooded and issue as adults early in July after passing the winter in the pupal stage.

THE LIFE HISTORY OF SCHIZONEURA LANIGERA HAUSM. By Prof. WM. B. ALWOOD, Va. Polytechnic Institute.

BREEDING records show that the so-called root and stem forms can be colonized from root to stem and stem to root at pleasure and also their natural migrations were observed.

Many of the agamic wingless females survive the winter exposed on aërial situations at Blacksburg, Va., and continue reproduction when the climatic conditions are favorable. Records show twelve generations of agamic viviparous females from May 12th to Sept. 20th. At this date winged agamic viviparous females were produced in all colonies under observation regardless of what generation it might be. These prove to be the migrant generation, and cannot be induced to remain quietly at rest on the apple plant under control conditions, but are extremely restless and fly away. Where they normally alight to produce their young could not be traced. Under confinement they produced four to six living young which proved to be true sexed individuals. These are small beakless creatures, with rudimentary lobes where the mouth parts should be. About two-thirds are females, and one-third males, both showing peculiar structural characteristics.

After copulation the females deposit a single egg which remains dormant over winter. From long observation it is concluded that in southern latitudes the agamic individuals continue an unbroken chain and that the oviparous form of reproduction plays no important role in life cycle. Only a small per cent of individuals actually present become winged, but on the contrary continue as normal agamic wingless lice.

THE PHYLOGENY OF THE NORTH AMERICAN EUCLEIDÆ. By Dr. HARRISON G. DYAR, U. S. National Museum, Washington, D. C.

THE Eucleidæ (Limacodidæ or Cochliopodidæ) is a family of moths with footless larvæ known as slug-caterpillars. I am acquainted with the life-history of twenty species, and a partial description of one other has been published out of a total of some twenty-eight or thirty species credited to this country. The larvæ offer many striking structural characters valuable in classification and I have prepared a chart showing the phylogeny of the twenty-one where larvæ are known. The first division of the phylum corresponds to the loss of the lateral tubercle of the first abdominal segment, with some accompanying characters. On this branch, the next division shows the origin of the stinging spine. On another branch the first division corresponds to the loss of the primitive first stage on the one hand and to the complete suppression of the many haired warts on the other. Many details are illustrated on the chart.

[To be published in Journal of the N. Y. Entomological Society.]

ON THE GENITALIA OF ANTS AND THEIR VALUE IN CLASSIFICATION. By W. H. ASHMEAD, U. S. Department of Agriculture, Washington, D. C.

REMARKS ON APHORISTICA. By Prof. EDWARD S. MORSE, Salem, Mass.

FOSSIL BUTTERFLY FROM THE BASE OF THE SEVERN FORMATION (CRETACEOUS) OF THE POTOMAC RIVER EXPOSURE. By P. R. UHLER, Baltimore, Md.

ON THE TYPES OF VERTEBRATE EMBRYOS. By Dr. CHARLES SEDGWICK MINOT, Harvard Medical School, Boston, Mass.

ON THE EMBRYOLOGY OF THE RABBIT. By Dr. CHARLES SEDGWICK MINOT, Harvard Medical School, Boston, Mass.

SOME NEW POINTS IN DINICHTHYID OSTEOLOGY. By Dr. C. R. EASTMAN, Museum of Comparative Zoölogy, Cambridge, Mass.

DESCRIPTION of cranial and dorsal shields of *Dinichthys pustulosus* from the Hamilton Limestone, shown by following characters to be the most primitive known member of the genus. (1) *Coccosteus*-like tuberculation. (2) *Coccosteus*-like pattern of cranial plates and arrangement of sensory canals. (3) Sinuous cranial sutures, and simple union by overlap of body plates. (4) Convexity of body from side to side, other Dinichthyids having a comparatively flat dorsal and abdominal covering. (5) Slight anterior emargination of dorsomedian plate. (6) Pineal perforation not yet detected (perhaps wanting?).

Review of cranial osteology of *D. intermedius*. The so-called "parietal" and "frontal" elements of Newberry and other authors declared not to exist, the area assigned to them being covered by the *centrals* as in *D. pustulosus* and uniformly throughout the family. Identification of the side plates of the body, heretofore supposed to be missing. The upper or transverse arm of the "clavicular" homologous with the anterior lateral of Coccosteus, and the inner branch of its bifurcated arm with the inter-lateral of the same genus. Outer branch of the bifurcated arm has articulated to its distal end a warped plate supposed to represent a modified branchiostegal apparatus. The term "clavicular" thus a misnomer, since the element has nothing to do with a shoulder-girdle, and there is absolutely no evidence that such a structure was present in the Dinichthyids.

The orientation of the clavicular was illustrated by diagrams. Its transverse arm overrides portions of the postero-antero-dorso-laterals, and rests against a flange at the base of the external occipital; hence it is probable there was but little action in a vertical plane between cranial and dorsal shields, and none in a transverse direction. The flat expansions of clavicular and suborbital must have projected in same plane as the dorso-lateral plates and head-shield respectively, thus greatly extending the transverse section of the body. As a result the more specialized species of the

Titanichthyida probably had almost ray-like proportions. Compression of body in a vertical plane was doubtless correlated with bottom-feeding.

The osteology of *Titanichthys agassizii* was illustrated by diagrams. The dermo lateral plates shown to be fused into a single piece, and sutures obliterated. The dermo-median shield referred by Newberry to *T. clarkii* believed to be in reality that of *T. agassizii*, and his supposed "hyoid or ventral plate" of *T. clarkii*, with its deep lateral incisions, substituted as the dermo median properly belonging to that species. Orientation of the plates indicates proves that Titanichthys was widest in transverse section of all fossil fishes yet discovered. At the same time, all the bones were foreshortened in an antero posterior direction. Newberry's supposed "eye-capsules" properly regarded as olfactory capsules; and his "coracoid" of *T. clarkii* shown to be only a fragment of the lower part of the clavicle. Titanichthys and Dinichthys retained in the *Coccosteidæ*, but Macropetalichthys made the type of a new family, *Macropetalichthidæ*.

[illegible] SAMPSON, [illegible] AND [illegible] SIMON HENRY GAGE, Ithaca, N. Y.

[illegible]

lowing April, or about seven months. It then emerges from the sand, and within a week or two builds its nest, lays its eggs and dies. This accounts for the fact that the American brook lampreys have never been seen by naturalists except at the spawning season.

Most works on zoölogy adopt the statement of Günther (Die Fische des Neckars, 1853) that the food of lampreys consists of insects, worms, crustacea and other aquatic forms, also the mucus and flesh, and possibly to some extent the blood and lymph, of fishes. During the last few years abundant opportunity has been afforded for determining the food of the lake lamprey (*Petromyzon marinus unicolor*), from Cayuga Lake. No food except blood has ever been found in the enteron in freshly caught specimens from the lake. One that attacked a catfish in the aquarium was examined soon after and a few shreds of striated muscle were found mingled with the blood. This is easily accounted for as a considerable hole was rasped in the flesh to get the blood.

As the brook lamprey has the same general armature of teeth and a rasping tongue as the sea and lake lampreys possess, it has always been assumed that the brook lamprey was likewise parasitic. Several brook lampreys were kept in an aquarium from October to April, that is, during nearly the entire time of transformation, and until the nest building began. Small fishes put in the aquarium for them to attack, if they wished, were never molested. Furthermore the lampreys remained under the sand like larvæ until within a short time before the nest building began. In April, six days before brook lampreys were found spawning, several nearly mature ones were obtained from the sand along the stream. These were put into the aquarium. They soon buried themselves, but after several days emerged fully mature and ready to lay their eggs. While the experiment with the aquarium made it highly probable that the brook lamprey remains under the sand until maturity, the probability was changed to a certainty by finding the animals in nature behaving in the same manner. Whenever the lampreys appear on the spawning grounds the alimentary canal is greatly atrophied and no food is taken. That is, during the only time when the brook lamprey swims freely in the water and when, from the development of its teeth and rasping tongue, it might be parasitic, no food of any kind is eaten.

As it has thus been demonstrated that the brook lamprey has no parasitic life, the question naturally arises as to the purpose of the buccal armature which is apparently as perfectly adapted for parasitism as its congener's, the lake lamprey, which is known to be parasitic.

The suctorial mouth is used for nestbuilding and for mating in all lampreys. Is that a sufficient explanation for its existence, and have the sea and the lake lamprey learned to make use of it for feeding in addition? Or was this suctorial mouth with its sharp teeth and piston-like, rasping tongue primarily for stealing food? If so, the brook lamprey has lost its parasitic habits; it has reformed, so to speak, and retains the suctorial mouth only for mating and for nest building.

HIBERNATION, TRANSFORMATION AND GROWTH OF THE COMMON TOAD (BUFO LENTIGINOSUS AMERICANUS.) By Prof. SIMON HENRY GAGE, Ithaca, N. Y.

IN the literature which the writer has been able to consult upon the hibernation of toads, it is either stated that the animal hibernates in protected places under leaves, boards, logs and stones, or no definite statement is made. From personal observation it is believed that hibernation takes place only in the ground, in situations which are not very moist, and which are not liable to be frozen. They emerge during the first continuous warm weather in spring. If, after emerging, a cold snap occurs, they seek the shelter of boards, leaves, etc., and it is this fact doubtless which has led to the belief that they remain in such places during the entire winter.

In burying itself in the ground, the toad does not take the ordinary method of going in head first, but reverses the process and backs in. The hind legs and caudal end of the body are used for digging and the front legs for pushing backward. From the movements of the body, the dirt caves in as the animal moves backward, and thus no sign of the hole is left.

With reference to the temperature which the animal can endure during hibernation, and its possible recovery if completely frozen, it was found that the body temperature of the toad in a freezing atmosphere was from [illegible] degrees higher than the surrounding medium. Toads found under frozen leaves and when the temperature was considerably below freezing, were not frozen and were able to crawl on the ice when uncovered. In one case the legs and skin were frozen, but not the internal organs, and that toad recovered, but when the animal was frozen solid it never recovered.

The time for breeding in the toad extends from the middle of April to the middle of June, in Ithaca. However, most of the eggs are laid during the last of April and the first of May. They are normally laid in a double string, one string for each oviduct. The number laid at one time varies greatly — from about five hundred to five thousand or more. In water from 10° to 15° centigrade the eggs hatch in about four days. In colder water the time is increased.

Under favorable conditions of food and temperature, the full growth of the tadpoles is attained in from forty to sixty days. The transformation has been observed in [illegible] days. After the hind legs are well developed, no food is taken and the active changes into a toad occupy three days.

The rate of growth of the toad, as well as of the tadpole, and the energy expended in transformation, is shown by weighing the animal at the beginning and at the end of the process. For example, a tadpole at its maximum size weighed [illegible] grams. Four days after the hind legs had at-

tained their full size and food was no longer taken, the weight was 110 milligrams. In this stage the tail did not show any signs of shrinkage. Two days later when the tail was only three millimeters long, and the little toad had crawled out into the air, the weight was 70 milligrams, or less than one-third the weight of the tadpole from which it developed.

Remembering that with man and the other animals which have been investigated, the proportion of water is greater the younger the animal, some careful experiments were made to see what part the loss of water played in the diminution of weight, in the toad. The moist weight of a tadpole at its maximum size was determined, and the dry weight after thorough desiccation. This showed that in the tadpole 91.6% of the live weight is due to water and 8.4% to tissue free from water. On the other hand, the dry weight of the just-transformed toad was 11.4%, and the water 88.6% of the live weight. There is then considerable loss in the per cent of water. The dry weight of the tadpole was 19 milligrams and of a toad into which a tadpole transformed only 8 milligrams. Thus the dry weight of the toad is less than half the dry weight of a tadpole similar to the one from which it came. This shows very strikingly that there is great loss of substance other than water in the transformation of toad tadpoles into toads. Shrinkage in volume during transformation has also been observed in other Anoura.

In the growth of the toad from the egg to the adult there is a steady increase from the egg to the maximum of the tadpole. During the transformation there is a marked shrinkage; but immediately after the little toad has passed the trials of transformation it is ready to take food, and under favorable circumstances its progress in size is quite rapid and within a few years it may be 50,000 times as heavy as the egg from which it came.

ON THE PISCINE ANCESTORS OF THE AMPHIBIANS. By Prof. THEO. GILL, Columbian University, Washington, D. C.

A HISTORICAL NOTICE OF ROSS' ROSY GULL, RHODOSTETHIA ROSEA. By JOHN MURDOCH, Public Library, Boston, Mass.

THE species was discovered in 1823 by Sir James Ross, at Alagnak, Melville peninsula, and was described by Richardson in 1825, but Macgillivray had published the name *Larus roseus* with a brief diagnosis in 1824. Up to 1881, only fourteen specimens were known, from Melville peninsula, Greenland, Færoes, England, Heligoland, Franz Josef Land, northeastern Siberia, St. Michael's, Alaska, and Kamchatka (?). The Point Barrow expedition (1881–83) found them in very large numbers, each autumn, flying northeast along the shore, and collected a good series. Fifteen additional specimens have since been taken, all from the Arctic regions, except one (Bering island). Nansen found them abundant in July,

near Hvidtenland (81° 38′ north). They undoubtedly bred there, but the main breeding ground is probably the land believed to lie not far north of Point Barrow. They probably always keep close to the lower edge of the ice pack.

[To be published in full in The Auk.]

THE WINTER FOOD OF THE CHICKADEE. By CLARENCE M. WEED, New Hampshire College, Durham, N. H.

THE paper records the results of a study of the food of the black-capped titmouse (*Parus atricapillus*) during the winter months. Forty-one specimens were studied between November, 1897 and March, 1898. The investigation showed that this bird feeds on a large variety of insect life during the winter. The most remarkable item in the food was that of the eggs of aphides, which constituted twenty-one per cent of the whole. Insects as a class constituted fifty seven per cent; spiders and their eggs six per cent; vegetable matter twenty-one per cent, of which, however, fourteen per cent consisted of buds and bud scales largely introduced by accident along with the aphid eggs. Ten per cent of the material could not be determined, and four per cent consisted of grit and other extraneous substances. This study gives additional evidence of the great usefulness of this familiar bird.

A RARE SPECIES OF WHALE. By Prof. ALPHEUS HYATT, Boston Society of Natural History, Boston, Mass.

THE specimen, which was the subject of this communication, came ashore on the beach at Annisquam, Mass., August, 1898. Having, through the kindness of Mr. Frank Parsons, secured the body and cleaned the skeleton, it has been found to be a species of Mesoplodon.

It is a female, just twelve feet in length, measuring along the body and making no allowance for the curvature of the sides. The two teeth, characteristic of the genus Mesoplodon, were found in the mid length of the lower jaw, but they were very small and not visible above the gum.

The only species described as having been found in the North Atlantic is *Mesoplodon bidens*, and it is probably a specimen of that species. This species has been recorded as occurring on the European coasts, but has been a very rare visitant of our shores.

VARIATIONS IN HUMAN BONES, WITH ILLUSTRATIONS FROM THE COLLECTION OF THE MEDICAL SCHOOL. By Dr. THOMAS DWIGHT, Harvard Medical School, Boston, Mass.

THIS was a lecture delivered before Sections F and H. The speaker dwelt especially on anomalous spines of the vertebral column.

THE FAUNA OF COLD SPRING HARBOR. By Dr. C. B. DAVENPORT, Harvard University, Cambridge, Mass.

THE Biological Laboratory of the Brooklyn Institute of Arts and Sciences, located at Cold Spring Harbor, L. I., is stationed on a reëntrant of Long Island Sound. The climatic conditions of this station differ greatly from those of a salient projecting into the ocean. First, the absence of sweeping winds permits the growth of dense woods down to the water's edge; and in the woods an abundant woodland fauna and flora. In the terminal moraines lying on each side of the harbor, springs arise, feeding lakes rich in fresh-water organisms — Volvox, Stentor, Hydra, Entomostraca and fresh-water Bryozoa.

Second, the marine fauna contains few or no stragglers — the animals which occur in the tow are natives. This circumstance makes the locality favorable for a study of marine œcology — the interrelation of organisms and their dependence on environment. Thus, Cold Spring Harbor is a peculiarly advantageous point for collecting animals as well as plants and for œcological work.

For marine collecting, the harbor offers sandy beaches, mudflats, eel-grass flats, pile-work and natural rock-work of boulders both at and below tide mark.

The fauna belongs to the Virginian province, there being fewer boreal types than in Vineyard Sound. Especially abundant among marine genera are, Hydractinia, Asterias, Leptosynapta, Molgula, Nereis, Caprella, Limulus, and crabs of several species. Over ninety species of Mollusca have been identified; of these many are very abundant.

NAPLES STATION: GENERAL DESCRIPTION AND NOTES ON METHODS OF WORK EMPLOYED THERE. By Dr. E. O. HOVEY, American Museum of Natural History, New York, N. Y.

THE first laboratory for the scientific study of marine life was established on the Bay of Naples, in the year 1872, by Dr. Anton Dohrn. Although some thirty stations have since been established on the same general plan in different parts of the world, the parent institution has always maintained its lead in the character and amount of work accomplished. The author spent several weeks there last autumn studying the methods in use for the proper preservation of soft-bodied marine animals for purposes of exhibition and study. The most popular feature of the Station is of course the aquarium, which consists of twenty-six tanks so arranged that no light enters the room except that which comes through the water in them. They contain all the hardy and many of the delicate forms of life for which the waters of the Bay of Naples are famous.

The chief function of the Station, however, is investigation, and this is carried on by the corps of nine regular officials attached to the institution, and the thirty or forty students from all over the world who come to the Station each year to profit by the wonderful opportunities it offers. More than one thousand men have studied at the institution since its opening. Outside of Naples, the Station is known to the public through the beautifully preserved animals which have been sent out to museums and colleges all over the world as well as through its series of valuable publications. The paper entered into some detail regarding the methods employed for the proper killing and subsequent preservation of marine animals.

GENERAL STATEMENT OF TYPES AND FIGURED SPECIMENS OF FOSSIL INVERTEBRATES IN THE AMERICAN MUSEUM OF NATURAL HISTORY. By Dr. E. O. HOVEY, American Museum of Natural History, New York, N. Y.

THE collections of the Geological Department of the American Museum of Natural History contain at least 7,000 types and figured specimens of fossils. By far the larger part of these came to the Museum through the purchase of the famous James Hall collection about twenty years ago, and comprises a large proportion of the palæozoic species of the New York series of strata, together with valuable series from Waldron and Spergen Hill, Indiana; and Racine, Beloit and other localities in Wisconsin and Minnesota. The Museum also possesses the F. S. Holmes collection, including many types from the Tertiary and Post-Tertiary beds of South Carolina; the Hall and Meek types from the Cretaceous of Nebraska; and the specimens which have been described by Prof. R. P. Whitfield in the Bulletin of the Museum.

INK AND PAPER FOR MUSEUM LABELS. By Dr. ROBERT T. JACKSON, Cambridge, Mass.

A LABEL is a record of the facts in regard to a specimen and as such should be of a permanent character. A good ink is of primary importance, as the average writing ink is worthless for permanent records. Carbon ink (India ink) is absolutely proof against fading and is the only black ink suitable for labels. Higgins' or other liquid carbon inks made for draughtsmen are satisfactory. A high grade ink may be prepared by mechanically breaking up a stick of India ink and then dissolving it in acetic acid. This is excellent for alcoholic material, not being affected by such conditions.

Rubber stamps are convenient in museum work, but the aniline inks in common use are fugitive. A good stamp ink can be made by grinding a

high grade of lamp black in glycerine. Add the lamp black gradually until the ink is of the consistency of thick syrup. Spread on a felt pad as in the case of other rubber stamp inks. This ink is not affected by alcohol or chemicals.

Red printing inks vary greatly in their permanency and care should be taken to secure the best for type or other labels.

The records of specimens are sometimes lost, especially in wet preparations on account of the inferior quality of paper used. The average paper is not sufficiently lasting for permanent records. A bond paper or linen record paper makes an excellent label. A heavily sized paper is liable to the attacks of insects (*Lepisma domestica*) which eat the label more or less extensively for the contained nutriment.

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BOTANY.

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ADDRESS

BY

WILLIAM G. FARLOW.

VICE PRESIDENT, AND CHAIRMAN OF SECTION G.

THE CONCEPTION OF SPECIES AS AFFECTED BY RECENT INVESTIGATIONS ON FUNGI.

THE fiftieth anniversary of the foundation of the American Association is a fitting occasion for a retrospective view of the different branches of science represented in our Society and one would be glad to hear from the lips of some botanist who was present at the first meeting of the Association, an account of the changes which have been brought about in the methods of botanical study and research and of the progress which has been made in North America during the past half-century. Fifty years, however, is a long time in the life of any individual, and of those who in 1848 were young, or comparatively young, even the most favored could hardly be expected to retain their scientific activity in 1898. On glancing over the list of members in 1848, one sees the familiar names of a number of botanists, including Ashmead, J. W. Bailey, Barratt, Jacob Bigelow, Buckley, Dewey, Emerson, Engelmann, L. R. Gibbes, Gray, B. D. Greene, Edward Hitchcock, Oakes, Olney, Pickering, Thurber, Torrey and Tuckerman. Not one of these leaders of American botany in their day remains to tell us of the Association in its infancy and to trace its development with the vividness which personal experience alone can supply.

It would be scarcely fitting in me to attempt to give a general sketch of the part which botany and botanists have played in the

life of the Association, nor, remembering the review of recent investigations in botany presented by Professor Marshall Ward at the meeting in Toronto last year, is it desirable that I should encroach on the ground so thoroughly and so interestingly covered by him. I may, however, on this occasion, be permitted to say a few words on a single question on which opinions have changed very much during the last fifty years and, avoiding a detailed history of the subject, treat it somewhat abstractly in its general bearings; for the question, you will admit, is one about which we should occasionally ask ourselves what is probably or possibly true, without, however, expecting in most respects to be able to reach positive conclusions. What do we mean by species? Do species really exist in nature or are they created by us for our own convenience? As I do not pretend to be in the position of a philosopher, but approach the subject as a very commonplace sort of a botanist, the word species as used by me means simply species as understood by the systematic botanist and indirectly by those working in other departments of botany who are obliged to depend to a considerable extent upon the limitations of species as defined by systematists.

The publication of the Origin of Species in 1859, a date which marks the fall of the old school and the rise of the new, is sufficient to show that it is not probable that any other period of fifty years in the future will have the same comparative historical importance, as far as the question of the conception of species is concerned, as the fifty years we are now commemorating. Had we asked any of the botanical members of the Association in 1848 what they meant by species they would have replied, most of them without reserve, a few with some hesitation, that in the beginning God created all species as He intended them to be and that by searching the naturalist could find them out. Just how they recognized species when they saw them would have been very hard for them to say, as they did not agree in their standards, but they would probably all have agreed in saying that the recognition of species was a matter of individual judgment, one's own judgment, of course, being better than that of any one else. The skeptic at that time could not have failed to notice the frequency with which what was home-made was confused with what was God-given. Before 1859, creation was one vast pudding in which the species had been placed like plums by an Almighty hand and

the naturalists sitting in a corner like greedy little Jack Horners, put in their thumbs and pulled out the plums and cried, "See what a great naturalist am I — I have found a new species!"

Probably very few of my hearers have any personal recollection of the time when not to believe that species were fixed and immutable creations was enough to make one a scientific and almost a social outcast. I recall but a few people whom I knew who held these orthodox views, for it was my good fortune to be a student in college at the time of the appearance of what was called "a new edition of the Origin of Species revised and augmented by the author," published by D. Appleton & Co. in 1864. By that time the novelty and audacity of Darwin's views had ceased to cause a cold shudder and certainly the students of my time were ready to swallow not only what Darwin had written but to add a few little theories of their own.

The young botanist of to-day will, I think, pardon me, although my contemporaries may not, if I give a short sketch of the Harvard Natural History Society in the sixties as showing not only how changed is the position of Natural History in American colleges, but also the attitude of college students at that day toward the then new doctrine of evolution. If the Society soon after my college days passed out of existence, its end could not be said to be untimely for the attitude not only of the university but of the scientific public towards the study of natural history had so changed that the old-fashioned society had no place. Those of you who go to Cambridge next Friday may perhaps see a dreary, barn-like sort of a lecture-room which now occupies the greater part of old Massachusetts Hall. In days gone by, the three upper stories of the hall served as dormitories, and the lower story was occupied by the rooms of the Natural History Society sandwiched in between those of the Institute of 1770, which then was pleased to consider itself to be a literary society, and the laboratory of the Rumford Chemical Society which, as it emitted none of the odors characteristic of chemical activity, must be considered in my day to have been moribund if not actually defunct.

The rooms of the Natural History Society would now cause a smile. From the low ceiling were suspended an alligator, a turkey buzzard and such other creatures as would not fit well in the wall-cases. In one corner leaned lazily a large cup-sponge, a receptacle for the dust which gravity constantly supplied and the

rejecta contributed at frequent intervals by the members. Around the walls was a very promiscuous collection of birds and mammals, some shot and prepared by past members, others the gift of so-called benefactors who, not knowing what else to do with them, turned them over to the Society. Quartz crystals and other showy, but not very valuable minerals hobnobbed with skeletons, one of which, at least, must have been very useful, if one could judge by the perennial absence of some of the limbs which had been removed, as was said, for study.

Botany was represented by a single cabinet, whose pigeon holes were filled with plants of New England, enriched by choice fragments of specimens collected by well-meaning persons in the Alps and by travellers in the Holy Land. The plants were arranged or rather shuffled, in the case according to the wishes or necessity of the curator of the time being. We were quite eclectic in our view of botanical classification, some pigeon-holes being arranged on the Linnæan system, some on the natural system and some apparently alphabetically. Whatever real value the collections may have had, once a year they were at least ornamental. Every year the members were photographed and the alligator, the turkey-buzzard, and the human skeleton were taken down and added to the group to show that we were really the Natural History Society and not the Hasty Pudding or the Phi Beta Kappa.

The old collections were long ago dispersed and the little which was of value is now incorporated with the different university collections. You may perhaps be curious to know what the members of the Society did. That is easily told. They all talked and some dissected cats. The talk was to a great extent about the origin of species and, no matter what was the subject of the papers announced for the evening meeting, it was not often that we adjourned without dropping into a discussion on evolution. Few had really read Darwin's book but all felt able to discuss the great scientific question of the day, in which respects we did not differ from some older and more learned people. Although the traditional man who is always on principle "on the other side" was not wanting, we were practically unanimous in our opinion. We all felt that a new day had dawned, that the old view of looking at species as fixed creations and ignoring as far as possible, the significance of their tendency to vary had been forever upset by Darwin and that hereafter we must look to evolution as brought about by natural selec-

tion to interpret species as we now find them. Not being well informed in regard to the history of scientific opinion, we assumed somewhat hastily that before Darwin all was darkness, and we did not trouble ourselves to go back and inquire whether there were not others who had had, at least, glimpses of the great truths of evolution, but even had we heard that there were some before Darwin who did not believe in the fixity of species, it would still have been true that it was Darwin's book by which, practically, the world at large was enlightened on the subject.

Forty years have passed and inasmuch as we are all evolutionists either of the Darwin school or some related school, the question suggests itself, Is our belief in evolution merely dogmatic like some of the theological doctrines which we believe thoroughly but which we do not allow to interfere with our daily life, or, as far as botany is concerned, has our belief modified the manner in which we treat what we call species? The mere fact that we now recognize that species have been derived from other species and are on the way to develop into still other species, would naturally lead us to be more liberal in our treatment of them systematically than in the days when variation was almost a crime against the Almighty. Certainly, with evolution as a key to guide us, our conceptions of genera and orders ought to be far more scientific than they were.

A species has been defined as a perennial succession of like individuals and, although no definition is perfect, I doubt whether a better definition of species has ever been invented. It is a peculiarity of definitions, however, that they all need to be defined. In the present case we must be told what is meant by the word perennial and what is meant by like. To the pre-Darwinian, perennial, of course, meant for all time. By the early Darwinians we are not told whether by perennial they meant a hundred, a thousand or a million years, but until we know, at least, approximately what is meant, we must still ask how long must be the succession of like individuals to establish a good species. Otherwise the whole matter of the distinction between a race and a species cannot be settled practically. If there is nothing definite in writings of the time of Darwin to explain the limits of the perennial succession, we should bear in mind that the object then was to bring out boldly the salient points of evolution as governed by natural selection and the illustrations used were taken almost exclusively from the higher animals and plants in which the lives of individuals are of such

duration that it was impossible to obtain accurately the records of a large number of generations in any case. Enough was shown and cited to show from the records of comparatively few generations a general tendency which it was assumed would be confirmed could the geological record be followed, and we can suppose that, so far as they considered the question at all, the early Darwinians took it for granted that the perennial succession needed to establish a species covered very long intervals of time. While one need not object to this method of reasoning, it is plain that the practical question of when a race or variety ceases to be a race and becomes a species was left open and it is questions of this sort which the systematist is constantly called upon to answer.

What could be learned only slowly and fragmentarily from observations and experiments on higher plants and animals might perhaps be learned much more easily could one experiment with organisms whose cycle of life is completed with great rapidity. For this purpose one might suppose that nothing could be better than bacteria, which are easily managed in the laboratory and whose development takes place with such rapidity that it is possible for the experimenter to watch the course of hundreds or even of a thousand generations in a comparatively short time.

The advantage to be expected from studying forms in which the development is very rapid is, however, made difficult for purposes of comparison by their extreme simplicity and the difficulty and, at times, impossibility of finding sufficiently marked morphological characters to guide us and, in the absence of such characters, the bacteriologist is often forced to base what he calls his species on physiological characters, including in that term zymotic and pathological action. By botanists, who are not specially bacteriologists, the so-called species of bacteria are not admitted to be species in the proper sense. Whether scientifically considered they are not as legitimately species as what are called species in speaking of the higher plants, is a very pertinent question. Any definition of species, to be scientifically accurate, must in its essential points apply to all plants and all animals and, if a species of flowering plant is a perennial succession of like individuals, it is hard to see why in bacteria a perennial succession of like individuals does not also constitute a species. That the individuals in bacteria are very different from the individuals in flowering plants is certainly true, but that does not affect the question of the validity of the

species in the former. As far as the perpetuation of morphological likeness of the individuals is concerned there is no doubt that it is, to say the least, as complete in bacteria as in flowering plants and the physiological constancy has been shown by competent observers to persist in some cases for hundreds of generations. That these many generations have been produced in months rather than in hundreds of years does not, it seems to me, affect the case.

When therefore, the botanist denies that physiological species are properly species, he is practically admitting that his own definition, the perennial succession of like individuals, is used by him in a special sense, and he does not seem to be aware that species as he limits them are artificial and not natural. The belief that species should be based on morphological rather than physiological characters rests on the assumption that the former are more likely to be inherited and thus show the ancestry while the latter are more likely to be the result of the temporary attempts of the organism to adapt itself to the environment. It is perhaps a question whether the grounds for this belief are as valid as has been supposed. We readily see morphological characters which have been inherited, but it is usually only by accident or experiment that we recognize the physiological or pathological qualities.

Let us turn for a moment from bacteria to Saccharomycetes whose characteristic function is to invert and ferment the different sugars. Here we have a group much more limited in number of species than the bacteria, but like them microscopic and rapidly growing. Although not long ago they were classified after a fashion on their morphological characters, the admirable investigations of E. C. Hansen and his followers have pointed out the important fact that these characters, taken by themselves, are less fixed, although the limits of their variation may be fixed, than certain physiological characters such as the maximum and minimum temperatures of growth, and especially the temperature at which spore-formation takes place. It is in these last-named characters rather than in the former that the specific distinctions in Saccharomycetes are sought by those who study that group specially.

The same objection is urged by botanists in this as in the case of bacteria that the so-called species are not species but races. We naturally ask, races of what species? There have been many attempts to determine the origin of the common Saccharomycetes and the question has been supposed more than once to be settled.

Without intending to imply that the question is not still open to investigation, I must admit that there does not yet seem to me to be any satisfactory proof to show from what higher forms Saccharomycetes have been derived. Although there can be no doubt that in the germination of spores of certain fungi, especially the Ustilaginaceæ, bodies are produced in abundance which not only closely resemble Saccharomycetes in shape but also, in some cases at least, are capable of producing alcoholic fermentation to a limited extent, it does not seem to me that that is by any means enough to warrant the opinion expressed by Brefeld that the Saccharomycetes are derived from and are degenerate conditions of Ustilaginaceæ. In fact, one has only to consult Brefeld's own writings to see that Saccharomycetes-like bodies are produced by the germinating spores of other orders of fungi than Ustilaginaceæ and it is known that, in some species, as in the genus Aspergillus and in certain Mucoraceæ, the budding cells which look like the Saccharomycetes, using the word in the limited sense, are also capable of producing alcoholic fermentation.

On the other hand no one has yet succeeded beyond a doubt in making the Saccharomycetes proper revert to a higher ancestral form. I say beyond a doubt, because the observations of Juhler, Joergensen and Johan-Olsen on the relation of Aspergillus, Sterigmatocytis and Dematium to Saccharomycetes have not been confirmed by other equally good observers, as Kloecker and Schioenning, and, for the present, at least, we must regard the observations of Joergensen and Johan-Olsen as affording still other instances of the fact that under proper conditions the germinating spores of many fungi produce bodies like Saccharomycetes, while they do not show conclusively that forms recognized by specialists as genuine Saccharomycetes can be transformed into fungi of other orders. They do, however, show that the views of Brefeld that the Saccharomycetes are derived from Ustilaginaceæ could, at the best, be only partially true.

Let us return to the question as to whether or not species of the Saccharomycetes, as defined by Hansen for instance, should be allowed to be called species in the proper sense of the word. Of course no one supposes that they have always existed in their present form and, although we have no exact knowledge of the ancestors of the present species, we naturally suppose that they were derived from some other higher fungi, as the expression goes.

Whether derived from one particular order of fungi or from several different orders, the species as we now see them seem to be constant in the sense that that word must be used in speaking of species of any group of plants. The shape of the cells in any given species, although variable to some extent, is constant within definable limits and although they have periods of rest and periods of activity their physiological action seems to be the same under similar conditions.

We might be justified, it seems to me, in regarding as races the Saccharomycetes-like forms which result from the germination of spores of higher fungi provided they continued to live an independent existence for a time and were not, as is more likely to be the case, merely accidental conditions depending on unusual or unfavorable conditions of germination, but the Saccharomycetes in the limited sense are constant, as far as constancy is to be expected in living organisms in general, they cannot be made to revert, as far as we know, and I therefore fail to see why they should not be admitted to be scientific species. The same is true of the physiological species of bacteria, meaning of course those which have been well studied and excluding the mass of ill-described and ill-known forms which abound in bacteriological writings. When a race has become so constant that it no longer reverts and we cannot tell from what species it came, it is no longer a race, but a species.

It may be objected, however, that both bacteria and Saccharomycetes differ from ordinary plants in a most important respect, viz., that there is a complete absence of sexuality and the reproduction is purely vegetative. There are a few botanists, to be sure, who think that there is a form of sexuality in Saccharomycetes, but botanical opinion, at present, is so overwhelmingly on the other side that to call the question an open one would require an explanation which time will not permit. It may be urged that in plants in which sexuality is wanting we have no right to speak of a perennial succession of like individuals, for it may be claimed, succession means by sexual generation only. This interpretation is very convenient if one wishes to ignore forms like bacteria and Saccharomycetes in the consideration of the question of species, but to exclude them on this ground is somewhat dangerous unless we are prepared to admit, off hand, that species are purely artificial.

It is the custom to speak of bacteria and Saccharomycetes as

degenerate forms. What is meant by this expression is not plain unless it means that, arising presumably from plants in which sexuality was present, they have become non-sexual. Undoubtedly sexuality is the rule in nature, but it should be borne in mind that it is not universal. I do not refer here to fungi like Ascomycetes and Basidiomycetes which, accepting the hasty conclusions of the Brefeld school, have been, even by a good many of our own botanists, included in the limbo of non-sexual, degenerate forms from which more recent observers are gradually rescuing them. I refer rather to species like *Rhodymenia palmata*, one of the commonest red seaweeds of the North Atlantic in which, so far nothing has been discovered but the non-sexual tetrasporic reproduction. This is not an isolated case and others will probably occur to my hearers. Furthermore we must admit that the number of species normally sexual but in which apogamy sometimes occurs has been perceptibly increased by the studies of botanists in recent years. In such cases as that of Rhodymenia it may be that the cystocarpic fruit really exists and will be found later; but, since botanists have searched for it in vain for many years, it must be very rare, and certainly, as far as we know it, the plant is non-sexual.

In regard to cases of apogamy we have not yet sufficient data as to their capacity for propagating themselves continually apogamously, although in such cases as that of *Chara crinita*, if we may judge by the distribution of the species in central Europe, there seems to be no reason to believe that they may not do so indefinitely. The not inconsiderable number of species of mosses, some of them common species, in which the male or female only is known and the number of marine algæ which, in spite of their frequency bear only tetraspores or at most bear cystocarps very rarely, should make us cautious in so defining what we mean by species as to imply that we consider that the perennial succession refers only to succession by sexual generation.

We cannot fail to notice an increasing tendency among cryptogamic botanists to give more and more weight to physiological characters in limiting their species. For some time we have been accustomed to think of the species of bacteria as largely physiological and we are gradually accustoming ourselves to the views of those who hold the same view in regard to species of Saccharomycetes. More recently still we find that in another higher order of fungi, the Uredinaceæ, experts are coming more and more to rely

on physiological characters. If in bacteria and Saccharomycetes we have plants which are generally recognized to be non-sexual, in Uredinaceæ the probability is that there is sexuality; at least, the probability is here much stronger than in the other two groups. By some the sexuality of Uredinaceæ is considered already proved, but admitting that the form of nuclear union demonstrated by Dangeard and Sappin-Trouffy and confirmed by some other botanists must have some important significance, not only in this, but in other orders of fungi where it occurs, there are reasons for not regarding the union in this case as representing true sexuality. On the other hand, although, no one has as yet quite proved it, there appear to be reasons for supposing that, in the æcidial stage, a form of true sexuality occurs comparable with what is known in some ascomycetous fungi. Time alone will show whether this present probability is a reality, but at any rate, the position of Uredinaceæ in regard to sexuality is undoubtedly very different from that of bacteria and Saccharomycetes.

One who takes up the recent descriptive works on Uredinaceæ is surprised to see the number of species which depend on physiological characters. The former method of describing the species of this order from the morphological characters of the teleutosporic, the uredosporic and æcidial stages was certainly sufficiently perplexing, but one almost gives up in despair on seeing species in which the different stages are identical in all respects, except that some of them, usually the æcidia, will grow only on certain hosts. Facts like this are of course only determined by artificial inoculations, although they may sometimes be suspected by the distribution of the different stages in nature. In this complicated state of things, more complicated than in any other order of plants, we are compelled to examine very critically the accounts of cultures made even by botanists of high reputation, and it is only natural that we should hesitate to give implicit confidence to negative results unless the observations have been repeated by other observers at other times and places. Even from scattered positive results one should avoid drawing too wide general conclusions. We may readily suppose that some of the supposed distinctions in the choice of their hosts by different Uredinaceæ will be proved hereafter not to be founded on fact, but, making all proper allowances for possible errors in observations and for hasty speculation in a field where speculation is so easy and accurate experiment so difficult, we have to admit that in a good many cases surprising

results have been confirmed by repeated observations and the tendency to split up species on physiological grounds becomes more and more marked.

As the subject is somewhat complicated it will be well to consider a few prominent cases by way of illustration. An instructive case is that of the Puccinia on *Phalaris arundinacea* referred to, among other subjects, by Magnus and Klebahn in papers published in 1894 and 1895. To the teleutospores was originally given the name *Puccinia sessilis* Schneider which was found by Winter to bear its æcidia on *Allium ursinum*. Later Plowright experimented with a species which grew on Phalaris whose teleutospores could not be distinguished from those of *P. sessilis*, but whose æcidia could be produced on *Arum maculatum* though not on Allium. To this physiological species Plowright gave the name of *P. Phalaridis*. Still later Soppit discovered that a Puccinia undistinguishable from *P. sessilis* and *P. Phalaridis* in its teleutospores produced its æcidia on *Convallaria majalis*. To this species he gave the name of *P. Digraphidis*. Had these observations not been confirmed by others we might have doubted whether Winter, Plowright and Soppit had not really experimented with the same species of Puccinia but, owing to some accident of their cultures, had succeeded in inoculating only different hosts whereas it might well be the case that the æcidia on the three hosts might by subsequent cultures prove to be the same and, in that case, *P. sessilis* would really be only an instance of a Puccinia which produces æcidia on three different hosts, not an infrequent case. The observations of Magnus showed that *P. Digraphidis* bore æcidia also on Polygonatum, and Maianthemum, genera closely related to Convallaria. So far as concerned Polygonatum and Maianthemum, Soppit and Magnus's observations were confirmed by Klebahn. The case is complicated by a difference of opinion as to whether the æcidium on Paris is connected with *P. Digraphidis* or whether there is not a fourth distinct species, *P. Paridis*, as believed by Plowright.

We need not stop to consider the further history of this complicated case as it is introduced here merely to illustrate the method and tendency of recent workers in this field.[1] The above named botanists who studied the species of Puccinia on Phalaris seem to

[1] Those interested in the subject may consult with profit an article by Klebahn, "Ueber den gegenwärtigen Stand der Biologie der Rostpilze in Botanische Zeitung of 16 May 1896."

agree in speaking of *P. sessilis*, *P. digraphidis* and *P. phalaridis* as distinct species, although Plowright considered *P. Paridis* to be distinct from *P. Digraphidis*, whereas Magnus considered the two to be what he calls adaptive races (Gewohnheitsracen) of the same species. Magnus speaks of the three species as biological species which he distinguishes from adaptive races, the latter including forms in which, although the æcidium may be produced on different hosts, it does not appear to be so frequent or so well developed on some hosts as on others, showing in the one case that the adaptation is more complete than in the other. Klebahn, although admitting that it is not of real importance whether one regards such forms as the Pucciniæ on Phalaris as species or races, nevertheless states that he sees no reason why they should not be considered to be genuine species rather than races.

Another instance in point is the group of æcidia generally known as species of Peridermium, which infest species of Pinus. It had for some years been recognized that the æcidial stage of the corticolous form of *Peridermium Pini* was not the same as that of the form on the leaves; but in recent years the subdivision has been carried much farther, owing to cultures made by Klebahn, Edouard Fischer, Rostrup and others. The former has distinguished at least seven species of Peridermium on *Pinus sylvestris* alone, whose uredo- and teleutospores are to be found in the species of Coleosporium which grow upon different genera of Compositæ, Scrophulariaceæ and Campanulaceæ. Although Klebahn is inclined to see minor differences in the shape and markings of the æcidial spores of some of the species, it must be admitted that the differences in some cases are so slight, both in the case of the æcidial spores and the corresponding teleutospores that, were it not that cultures show the connection between the form of one host with that on another to the exclusion of other hosts, it is hardly likely than many botanists would consider them as distinct species.

The most suggestive Uredinaceæ for our present purpose are the different species of Puccinia which attack grains and other grasses, for a knowledge of which we are indebted to the researches of Eriksson and Henning in Sweden, whose work is certainly a model of careful investigation. I take it for granted that most of my hearers are already acquainted with the character of the work in question and we need stop to consider only those points which bear upon the subjects we are discussing. Of the three common

rusts which affect grains, *Puccinia graminis*, *P. rubigo-vera* and *P. coronata*, the æcidia are to be found respectively in *Æcidium Berberidis*, *Æc. Asperifolii* and *Æc. Rhamni*, according to the previously accepted view in regard to those species. Judging by the morphological characters of the teleutospores and the uredospores alone, these three species occur on a large number of different grasses. In making inoculations to ascertain the facts in regard to the æcidia of the species, Eriksson and Henning found that what was supposed to be *P. graminis* growing on *Phelum pratense* and *Festuca elatior* had no æcidia, and they described this form under the name of *P. Phlei-pratensis*. *Puccinia coronata* is separated into two species: *P. coronifera* and *P. coronata*, the former having its æcidium on *Rhamnus catharticus*, the latter with æcidia on *Rhamnus Frangula*, with perhaps two other forms to be separated from the old *P. coronata*. *Puccinia rubigo-vera* is separated into three species: *P. glumarum*, *P. dispersa* and *P. simplex* — the distinctions based largely on the presence or absence of the æcidium although there are also certain differences in the habit and color of the other stages. The three original species are split up into seven species, besides two uncertain forms, characterized in the main by physiological characters. Furthermore, of *P. graminis*, six specialized forms are enumerated, characterized by differences in the inoculating capacity of the uredo- or teleutospores on different hosts. The other species also have their specialized forms, the total number being, I believe, twenty-eight. We may consider the specialized forms to be races and, in that case, certainly, we shall have to agree with Eriksson and Henning in considering their seven species as species rather than races. The important point is to know whether the differences observed are temporary and accidental or permanent. It is too much to ask for the confirmation of the results of these two experimenters just now, for their work is recent and has been carried so far beyond that of previous experimenters that it must require a considerable number of years before we could expect the work to be repeated by others. So far as the experiments have been repeated, as in the case of *P. coronifera* and *P. coronata*, it has been confirmed.

Enough has been said to show that the conception of species by those who are doing the most advanced work in fungi is much more flexible than it used to be and significance is to be attached to the fact that the number of those who, as viewed by the typi-

cal systematic botanists, hold very heterodox views is increasing. The explanation is to be sought in the fact that descriptive botany in certain groups of plants has reached a point where the ordinary morphological characters no longer suffice to classify what we know or wish to know about the plants themselves. It was my privilege eleven years ago to address what was then the biological section of the Association on a subject somewhat related to that of to day and my closing sentence then was: "Following the prevailing tendency in business affairs the question they [botanists] ask of plants is not so much, 'Who is your father and where did you come from?' as, 'What can you do?'"

The tendency noticed eleven years ago is even more marked at the present day. As compared with the times of which I attempted to give a sketch in my opening remarks, I think we may truly say that whatever may be the case in zoölogy, in botany theoretical considerations with regard to evolution play a much less important part than they used to. In the case of such plants as Lycopodiaceæ, Equisetaceæ and their allies and of certain orders of phanerogams, the ancestral question naturally remains as important as ever; but, although papers on other orders of plants accompanied by hypothetical genealogies and family trees of the banyan type appear at not infrequent intervals in botanical journals, they are quite overshadowed in general interest by the papers on cytology, life histories and physiology. That was not the case in the sixties when nothing compared in interest with the question of the origin of species. While we cannot be too grateful to Darwin for having opened our eyes to see the value of evolution in general, the majority of the active botanists of the present day find too many other pressing questions to be solved to be able to dwell on evolution to the same exclusive extent as did the botanist of the last generation.

Our definition of a species included two terms which required further explanation. We started out in the hope of finding some light as to the approximate length, or at least, the approximate minimum of the length of time which is needed to transform a race into a species, hoping that perhaps those plants in which the development of the individual was rapid might show that, in a comparatively short space of time, a race might be actually observed to become fixed and be considered a species, a fact which certainly could not be so well ascertained by direct observation in the study

of the higher plants alone. You will notice that, like the obliging shopkeeper, I have not given you exactly what you expected, but have offered you instead something else perhaps just as good, if not better. If I have not been able to tell you that in such simple and quickly growing plants as bacteria and Saccharomycetes new species can be produced from old ones in a comparatively short time, a consideration of some of the peculiarities of such plants has brought out the modifications which have taken place in the views of a good many as to specific limitations, which is in part an answer to our primary question, What do we mean by a species?

It may be added that although some of the species of lower plants may be transformed in various ways by artificial cultures, on the whole we are quite as much struck by their comparative constancy in important respects as by their tendency to differentiate. In Uredinaceæ there is a tendency to form adaptive races, which is greater than was formerly supposed, but whether the tendency is greater than would be found in some higher plants were they studied as carefully as have been the Uredinaceæ, is perhaps a question. Parasites, as a rule, are more plastic and more sensitive to changes of environment than other plants and their impressionability, if I may use that word, might be expected to accentuate their power of specific transformation. It cannot be denied that there is a general suspicion (to say knowledge would be too strong) that the lower plants become specifically changed more easily and quickly than the higher, but, although that is what we should expect from their more rapid individual growth, I am not able to cite any actual observations which can settle the question for, as you know, the school of botanists which may be called the school of ready transformationists have a fatal tendency to accept unskilfully-conducted or otherwise faulty observations as convincing proof. Others, it is to be feared, err on the other side and are not sufficiently ready to admit metamorphoses in different species of the lower plants. Probably the truth lies between the two. The metamorphoses to which I now refer are, of course, in the normal cycle of individual development and should not be confused with the differentiation into races and species, but of necessity our views as to the latter must be influenced to some extent by our attitude towards the former.

If we turn to the second word of our definition which needed explanation and attempt to say what is meant by like individuals, we

find ourselves wholly at sea. Even if we agree that the likeness must be morphological and not physiological, that does not help the matter at all. No two individuals are ever absolutely alike in morphological characters and the question is one of comparative likeness only. Systematists may agree that certain characters are more important than other characters, but they would never agree as to what characters are important enough to be regarded as specific in comparison with those which are only racial. In fact, when we come to the point, we find that most systematists do not in practice distinguish species from races on the ground that the former are practically constant, whereas the latter are not, but rather on the ground that they regard the characters which they use to distinguish species as more important than those which they are willing to accept as merely racial.

But what is more important and less important is a question not only of individual opinion at any given time, but it is also a question which depends on the means of analysis at our disposal and these change from time to time. Surely there never lived a better systematist than Elias Fries, and, at the time of its publication in 1821-1832, his Systema Mycologicum was certainly a masterpiece. If the species described by him in genera, such as Sphæria, for example, which were then considered valid, are no longer recognized as such, it is not because in limiting his species as he did Fries did not employ with remarkable skill the same scientific principles of classification as the mycologists of to-day, but mainly because the modern application of the microscope to the study of the spores and some other characters has brought out facts unknown in the beginning of the century. The species of Fries have been split up and changed in many respects and while we feel sure that the modern classification, thanks to improved microscopes, is an improvement on his, who shall dare say that hereafter some at present unknown and unsuspected method of analysis may not furnish facts which will overturn our present system?

I should feel that I ought to apologize for bringing up a subject so very, very threadbare, were it not that some botanists shrink from acknowledging the fact that what we botanists call species are really arbitrary and artificial creations to aid us in classifying certain facts which have been accumulated in the course of time, and nothing more. So long as we entertain even a lingering suspicion that they are anything more, systematic botany will not be

able to accomplish its real object, which is certainly very important, We are all convinced, theoretically at least, that not only are all plants gradually changing and sooner or later will no more be what they now appear to us to be, than they are now what they were in times past, and we also know that the means which we have of studying them are changing as well. Our so-called species are merely snap-shots at the procession of nature as it passes along before us. The picture may be clear or obscure, natural or distorted, according to our skill and care; but in any case, it represents but a temporary phase, and in a short time will no longer be a faithful picture of what really lies before us, for we must not forget that the procession is moving constantly onward and at a more rapid rate than some suspect. Better cameras will be invented and when another generation of botanists snap off their pictures, they will undoubtedly look back with pity, if not with contempt, on our faded and indistinct productions.

Whether or not species really exist in nature is a question which may be left to philosophy. Our so-called species are only attempts to arrange groups of individual plants according to the best light we have at the moment, knowing that when more is known about them our species will be remodelled. We should not allow ourselves to be deluded by the hope of finding absolute standards; but it should be our object to arrange what is really known, so that it can be easily grasped and utilized. Utility may perhaps sound strange and may seem to some to be a very low aim in science, but in the end utility will carry the day in this case, for systematic botany is a means, not an end. It is true object should be to map out the vegetable kingdom in such a way that all known plants are grouped as clearly and distinctly as possible in order that the horticulturist, the forester, the physiologist, may be able to obtain the facts needed by them in their work. Our present knowledge may not be sufficient to enable us to draw all the contours sharply or to lay down accurately all the lines, but our work certainly should not be blurred by subtleties and purely metaphysical refinements. The best systematist is not he who attempts to make his species conform to what he believes to be the ideal of nature, but he who, availing himself of all the information which the histology, embryology and ecology of the day can furnish, defines his species, within broad rather than narrow limits, in clear and sharply-cut words which can be readily comprehended and do not force one to resort

to original, and perhaps single specimens, to learn what the author of the species really meant.

The end which we all wish ultimately to reach is a knowledge of how living plants act, but in the process of obtaining this knowledge it is necessary to call to our aid, not only the physiologist, but also the systematist and the paleontologist — for there are many questions ultimately to be settled by the physiologist for which the information furnished by the systematist must serve as a basis and the geological succession must be supposed to throw some light on present conditions. It is no disparagement to systematic botany to say that it should look towards physiology as its necessary supplement for, on the other hand, physiology must lean on systematic botany in attempting to solve many of its problems, and the scientific basis of both rests on histology, morphology, in the modern sense, and embryology. The qualifications needed in a physiologist are so different from those required in a systematist that no one is warranted in speaking of one as of a higher grade than the other. If it has become the fashion in some quarters to assign the systematist to a secondary place, it cannot be attributed to the fact that his work is necessarily inferior in quality, but is rather due to the fact that, in too many cases, systematists have failed to recognize what should be the legitimate aim of their work.

The utilitarian tendency is well shown by what has been said in speaking of bacteria and Saccharomycetes. Did time permit and were the subject not one which would not readily be followed with patience by an audience at this late hour, other instances, especially in Ustilaginaceæ might be given to illustrate further the point in question. The bacteriologist bases his species on grounds which he thinks best suited to enable him to group together intelligently the plants he is studying and it is nothing to him that others say that his species are not species, but races. After all, the question whether certain forms are to be considered species or races, is in many cases, merely a question of how much or how little we know about them. The races of one generation of botanists often become the species of the next generation, who, as they study them more minutely and carefully, discover constant marks not previously recognized. As systematic botany develops in the future, it may very well become the study of races rather than species as we now consider them. In some cases, as in the Uredinaceæ, the time may be not far distant when this condition of things

will be reached. We also feel warranted in believing that hereafter physiological characters will assume even a greater importance than at present in the characterization of species. If there are some among my hearers who do not agree with me as to the importance to be attached to utility, I think that we shall all agree that in discussing the work of botanists in other departments than our own, it would not be wise to exact a rigid conformity to our individual conceptions of species as distinguished from races.

PAPERS READ.

[ABSTRACTS.]

THE CARPOSPORIC TYPE OF REPRODUCTION OF THE RHODOPHYCEÆ. By BRADLEY M. DAVIS, Chicago, Ill.

THE paper, after a consideration of certain work of the author upon the genera Champia, Ptilota, etc., discussed the cytological problems concerned with the carposporic type of reproduction found in the Rhodophyceæ. These considerations concerned themselves chiefly with the theories of Schmitz and Oltmann.

THE COMPARATIVE MORPHOLOGY OF THE PISTILS OF THE RANUNCULACEÆ, ALISMACEÆ AND ROSACEÆ. By ERNEST A. BESSEY, Lincoln, Neb.

As a result of investigations extending over a period of three years, the uniovulate pistils of the Ranunculaceæ are found to have a peculiar development. Each pistil originates as an open leaf in whose axil grows a mass of cells from the receptacle, forming one side of the cavity of the pistil, whose other sides are formed by the laminæ and by the curved distal portion of the carpel. Into this cavity grows a median ovule from the axillary mass of receptacular cells.

Sagittaria and Alisma have practically the same structure as the foregoing except that the ovule in the latter grows directly from the receptacle instead of from an outgrowth of it. This appearance is concealed in growth by the proportionally great growth of the pistil.

Potentilla and Fragaria in the Rosaceæ show a course of development very similar to the foregoing; an open pistil is formed, but the ovule, instead of growing from the receptacle, grows from the thickened edge of one or the other of the laminæ. Apparently this is a modification of the Ranuncular type in which the projecting mass of cells from the receptacle has become somewhat lateral, instead of remaining median, and has become a part of the lamina toward which the displacement has occurred.

This close similarity of development apparently supports that system of classification of the Angiosperms in which there are three diverging lines of development having a common origin: one with the Alismaceæ first, being the Monocotyledoneæ, the two others forming the Dicotyledoneæ, one with the Ranunculaceæ lowest being the Thalamifloræ and passing up to the Heteromeræ and to the Bicarpellatæ; the other, with the Rosaceæ lowest, being the Calycifloræ, and passing up to the Inferæ.

The Lycopodinæ among the Pteridophyta show, by the arrangement of their sporangia and by the strobilar arrangement of the sporophylls, closer affinities to these lowest of all angiosperms than do any other Pteridophyta.

THE ORIGIN AND HOMOLOGIES OF BLEPHAROPLASTS. By HERBERT J. WEBBER, Washington, D. C.

THE blepharoplasts of Zamia arise *de novo* in the cytoplasm of the generative cell appearing first as very minute granules upon which a few kinoplasmic rays are centered. The rays seem to connect intimately with the reticulum of the cytoplasm and apparently are simply accentuated portions of the latter.

In the bursting of the blepharoplast, during the formation of the ciliferous band, the outer membrane of the blepharoplast bursts into granules and these later become organized into a continuous membrane, which bears the cilia.

The blepharoplasts present striking analogies to centrosomes, but in the present state of our knowledge we are hardly justified in considering them to be homologous organs.

THE BLEPHAROPLAST IN THE SPERMATOGENESIS OF MARSILIA. By Dr. W. R. SHAW.

BELAJEFF[1] has described bodies found in the spermatids of ferns and Equisetum, which we may regard as homologous with the cycad blepharoplasts described by Webber.[2] The writer[3] has found that in Onoclea the blepharoplasts occur not only in the spermatids, as described for the ferns by Belajeff, but also before and during the cell division which leads to the formation of the spermatids.

In the development of the male prothallia of *Marsilia vestita*[3] from the microspore, neither blepharoplasts nor other centrosome-like bodies are found in any of the six cell-divisions or resting stages preceding the formation of the primordial spermatogenous cell ("central cell") of each of the two antheridia. Nor are any such bodies found in that cell or during its division.

The first appearance of centrosome-like bodies is in the spindle poles during the telephase of the second division of the spermatogenous tissue. After this division is complete (*i. e.* in the "grandmother cells" of the spermatids), each of these bodies grows into a pair of *blepharoplastoids* which increase in size. They remain together and move into the outer region of the cytoplasm, and disappear during the third cell-division.

[1] Wl. Belajeff. Three preliminary papers in the Berichte d. deutsch. Bot. Gesell. 1897.

[2] H. J. Webber. Three preliminary papers in the Botanical Gazette. 1897.

[3] W. R. Shaw. A preliminary paper in the Ber. d. deutsch. Bot. Gesell. 1898.

During the anaphase or telephase of this division, there are found, at the poles of the spindle, new bodies. After the division (*i. e.* in the "mother-cells" of the spermatids), each of these bodies develops into a pair of *blepharoplasts*. The blepharoplasts separate, at the same time increasing in size, and move to opposite poles of the cell, where they remain throughout the final division leading to the formation of the spermatids. The further development resembles very much that of the blepharoplasts in the spermatozoids of the cycads.

The blepharoplastoids are homologous with the blepharoplasts. Their disappearance, followed by the development of the blepharoplasts, is a point which resembles the disappearance and formation *de novo* of centrosomes as described recently in the segmentation of the eggs of certain animals.

A clue to the identity or distinctness of the blepharoplast and centrosome should be sought in the antheridia of lower plants, such as the liverworts and Fucaceæ, in which centrosomes have been described.

OBSERVATIONS ON THE RELATIVE MOISTURE CONTENT OF FRUIT TREES IN WINTER AND IN SUMMER. By Prof. C. S. CRANDALL, Fort Collins, Colo.

THE necessary removal of a portion of an apple orchard, fifteen years old, afforded an opportunity to test the correctness of the commonly accepted belief that trees contain much more water in summer than in winter.

The trees were grown under the same conditions on a gentle north slope, on rather low ground. The soil was a gravelly loam and had not been irrigated for five years, but had been kept sufficiently moist by seepage from higher lands. All the trees were healthy, and had made a normally vigorous growth the previous season.

Five trees were used for the test of winter moisture. Three were taken late in the afternoon of January 15, 1897, and two early the following morning. From each tree, as soon as cut, a trunk piece 45 cm. in length was taken, and also a bundle of branches cut at approximately 2 cm. in diameter and representing about one-fourth of the branch surface. These were immediately weighed and placed on the floor of an upper room in the laboratory, where they have since remained.

The trunk pieces ranged from 9.5 cm. to 12 cm. in diameter, with an average for the five of 11 cm. They were numbered 1 to 5, and the bundles of branches, 1a to 5a. Weights were taken eight times between the first weighing and the close of the year 1897, both trunks and branches showing a gradual loss of moisture.

Three trees were used for the test of summer moisture. These were taken August 3, 1897. Trunk pieces 45 cm. long and averaging 12 cm. in diameter were cut, weighed and placed with the others. Bundles of branches were also prepared similar to those taken in January, except

that, after weighing, the leaves were carefully removed, and branches and leaves weighed separately.

To distinguish between the two lots, the summer cut trunks were numbered S1, S2 and S3. The branches of No. S1 were marked S1a, and the leaves of S1a, S1b and so on.

It should be explained that the room in which the material under test has been kept is heated, in winter, by hot water. The winter temperature was quite constantly at about 22°C. The summer temperature has been quite variable, ranging between 10°C. and 35°C.

The figures to follow represent simply a determination of moisture lost by air drying under the conditions given. Constant care was taken to prevent loss by breakage of twigs or by detachment of pieces of bark.

In the following tabulation only the original weight and the weight of July 11, 1898, have been indicated, with the number of days intervening between the two weighings, and the per cent. of loss.

NO.		WHEN TAKEN.	WEIGHT WHEN TAKEN, HECTOGRAMS:	WEIGHT JULY 11, 1898, HECTOGRAMS:	NO. OF DAYS BETWEEN FIRST AND LAST WEIGHT.	LOSS %
1	Trunk	Jan. 15, 1897	63.361	38.856	542	38.67
2	"	" " "	40.681	24.752	542	39.15
3	"	" " "	55.353	32.247	542	41.74
4	"	Jan. 16, 1897	39.122	23.866	541	38.99
5	"	" " "	48.548	29.235	541	39.78
1a	Branches of 1	Jan. 15, 1897	65.451	37.456	542	42.77
2a	" " 2	" " "	40.681	24.858	542	38.39
3a	" " 3	" " "	44.721	26.081	542	41.68
4a	" " 4	Jan. 16, 1897	47.961	28.969	541	39.62
5a	" " 5	" " "	52.588	30.209	541	42.55
S1	Trunk	Aug. 3, 1897	79.130	49.647	342	37.25
S2	"	" " "	45.465	30.280	342	33.39
S3	"	" " "	49.682	29.430	342	40.76
S1a	Branches of S1	Aug. 3, 1897	34.976	19.454	342	44.37
S2a	" " S2	" " "	42.241	23.016	342	45.51
S3a	" " S3	" " "	26.400	14.848	342	43.75
S1b	Leaves of S1a	Aug. 3, 1897	14.387	6.059	342	57.88
S2b	" " S2a	" " "	21.758	10.064	342	53.74
S3b	" " S3a	" " "	18.816	8.575	342	54.42

The three lots of leaves have shown constant weights since January 8. The branches and trunks of both winter and summer lots still show slight changes, and further weights will be taken.

That the decrease in weight is now very slow is shown by the fact that the average loss of the winter trunks between January 8 and July 11, a period of 184 days, was only 3%. During the same period the summer trunks showed an average loss of 2.8%, but between the weight taken March 28, and the last weight, July 11, the average loss was reduced to .5%.

The tabulation shows that, of the five trunk pieces taken on January 15 and 16, 1897, after the first three numbers had air dried for 542 days and the last two for 541 days, the average loss was 39.666%.

The trunk pieces taken August 3, 1897, show an average loss, in a period of 342 days, of 37.133%.

Winter branches for the same period as the winter trunks show an average loss of 41.102%.

Summer branches, for the period of 342 days, show an average loss of 44.543%.

The three lots of leaves show an average loss, in 342 days, of 55.346%. These figures indicate that the trunks taken in winter contained 2.533% more moisture than those taken in summer; but we are here comparing unequal numbers. If we take the average of the three winter trunks showing the greatest loss, and compare this average with that of the three summer trunks, we find the winter moisture exceeds the summer moisture by 3.09%, or if we take the three winter trunks showing least loss, the loss excess is still 1.803% in the same direction.

The average of the five bundles of winter branches compared with the average of the three bundles taken in summer, shows the summer bundles to contain 3.441% more moisture than the winter ones.

Taking equal numbers, as with the trunks, the average of the three winter bundles showing greatest loss, compared with the average of the three summer bundles, shows the summer bundles to contain 2.210% more moisture than the winter bundles, while the three winter bundles showing least loss, compared in the same way, give 4.48% as the excess of moisture in the summer branches.

It might be expected that the summer branches would show greater loss of moisture than the winter ones, on account of the succulent nature of the unripened growth of the season.

While the trunks and branches do not show perfectly constant weights, the loss is now so small that it is believed final weighings will not materially change the figures, and they may be taken as approximately correct for the trees under test.

From the trial made, it seems reasonable to conclude that there is hardly an appreciable difference between the winter and summer moisture contained in apple trees, at least, as grown under Colorado conditions.

SOME OBSERVATIONS BEARING UPON THE SYMBIOTIC MYCOPLASM THEORY OF GRAIN RUST. By H. L. BOLLEY, Experiment Station, Fargo, North Dakota.

THE paper reviews the work of Dr. Jacob Ericksson upon which that author has proposed a new theory for the origin of rusts in cereal grains and other grasses. Mr. Bolley dissents from the views given by Dr. Ericksson and indicates that the conclusions drawn by that author from many of his experiments do not of necessity follow. The original matter of the paper represents a review of the author's work and observations covering a period synchronous with the work of Ericksson, and recounts the results of numerous germination tests made upon uredo-spores and upon æcidio-spores of certain species of rusts to determine the vitality of these spores. An outline is also given of the methods, and results obtained by the growth of cereals to maturity in rust-proof cages. In opposition to Ericksson, the author holds that there is much evidence that all infection to new grain plants comes from without, and that there is no evidence of an internal symbiotic rust life. Incidentally it was observed that the germ tubes of the uredo-spores may enter not only by way of the stomata, but also direct through the cuticle of wheat and oat plants.

(Paper to be published in Centralb. f. Bakt. 2 Abt.)

STARCH DISTRIBUTION AS AFFECTED BY FUNGI. By Prof. BYRON D. HALSTED, New Brunswick, N. J.

THE leaves to be studied are placed in 50% alcohol to remove the chlorophyll and then passed to a weak solution of iodine. In a few hours, if the leaf is thin, the starch responds to the iodine test and is located by the blue color that it assumes.

Dried leaves of *Agrimonia mollis* (T. & G) affected with *Peronospora potentillæ* D.By., showed a bluish color in irregular patches that correspond quite closely with the areas bearing the conidia of the mildew.

The dried leaves of the hog-peanut (*Falcata comosa* (L.) or *Amphicarpæa monoica* Ell.) infested with *Synchytrium decipens* Farl. were interesting subjects, because of their extreme thinness and the fact that the fungus is without hyphæ and therefore thoroughly localized in the infested cells of the host. The starch-bearing tissue is immediately around the spore cysts and as these are usually located along the lateral veins the blue lines correspond closely to the directions of the veins.

In leaves of *Amarantus retroflexus* L. having several large circular patches of *Cystopus bliti* Biv. the starch was found most abundant as a border around the diseased spots.

The most striking study was had with the mandrake leaves infested with *Puccinia podophylli* Schw. The amount of starch was abundant in

the well defined areas while none was met with beyond the small veins that served as boundaries to the infested portions. Here again it was observed that the diseased portions bear some relationship to the main veins. There was less starch in the center of a rusted mesh than near the vascular outline.

Smutted corn leaves when tested showed much starch in the bullate portions characteristic of the affected parts.

Stems infested with fungi showed the same presence of starch as the leaves. Thus the hypertrophied flower stalks of the cultivated radish when sliced and placed in the iodine solution became blue, while similar sections taken from a healthy portion of the same stem showed but a small amount of starch.

The results were almost the same as the above with the healthy and cystopusized stems of purslane (*Portulaca oleracea*).

The ordinary red cedar stem contains but a small amount of starch, but the galls made by the *Gymnosporangium macropus* Lk. abound in it and it is distributed in lines radiating like a fan from a common center.

The galls of the peach roots are simply gorged with starch and thin sections of them, in iodine, turn to a dark blue color.

Turnips affected with the club root fungus (*Plasmodiophora brassicæ* Wor.) carry a large amount of starch, while the healthy tissue is comparatively free from it. In like manner the tubercles of leguminous plants, due to *Rhizobium leguminosarum* Fr., contain much starch, while the roots bearing them are comparatively free from it.

The infested fungus seems to induce a change in the surrounding tissue more or less apparent with the storage therein of food material as shown by the starch test as above demonstrated.

SOME EXAMPLES ILLUSTRATING MODES OF SEED DISPERSION. By W. J. BEAL, Agricultural College, Washington, D. C.

POPULAR paper illustrated by specimens showing various adaptations of seeds and fruits for distribution by wind, water, birds, quadrupeds, etc. [To be printed in American Naturalist.]

THE INFLUENCE OF ETHER UPON THE GERMINATION OF SEEDS AND SPORES. By Dr. C. O. TOWNSEND, College Park, Maryland.

IT has been found that an atmosphere of ether exerts considerable influence upon the germination of seeds and spores. If seeds and spores are surrounded by an atmosphere containing but little ether ($\frac{1}{10}$ to 1 cc. per litre) the time of germination will be shortened while in an atmosphere containing considerable ether (5 to 10 cc. per litre) the time of germination will be lengthened. If the atmosphere contains too much ether the seeds and spores will fail to germinate. If seeds and spores

have not remained too long in a strong atmosphere of ether and are removed to an ether-free atmosphere they will germinate in the same length of time as is required by seeds and spores under normal conditions. It should also be noted that seeds and spores differ among themselves in their ability to resist the influence of ether.

THE TOXIC ACTION OF A CERTAIN GROUP OF SUBSTANCES. By Dr. RODNEY H. TRUE, Madison, Wis.

ZOÖLOGISTS and botanists have long regarded salts, of which NaCl and KNO_3 are commonly-used representatives, as being essentially lacking in toxic action and operating only through their osmotic activity.

In this study cane sugar was assumed to be a purely osmotic agent and the first concentration in which the majority of a number of filaments of Spirogyra survived, after an exposure of twenty-four hours, was regarded as the measure of the osmotic action which the alga can endure. This boundary concentration was found to lie at $\frac{1}{4}$ grm. molecules per litre.

Assuming the action of the other substances studied — glycerine, KNO_3 and NaCl, — to be likewise purely osmotic, the boundary concentrations were calculated to lie as follows: glycerine, $\frac{1}{4}$ grm. mol. per liter; NaCl, 0 47 grm. mol.; KNO_3, 0.45 grm. mol. per. liter. The actual boundary concentrations found by the method used for sugar were: glycerine $\frac{1}{4}$ grm. mol. per liter; KNO_3, $\frac{1}{16}$ grm. mol.; and NaCl $\frac{1}{10}$ grm. mol. The concentrations producing plasmolysis were as follows: glycerine, $\frac{1}{3}$ grm. mol. per liter; KNO_3, $\frac{1}{4}$ grm. mol.; NaCl, $\frac{1}{4}$ grm. mol.; sugar $\frac{1}{3}$ grm. mol.

The boundary concentration for glycerine lies at a concentration greater than that of the cell sap, and probably causes death by osmotic action. The boundary concentrations of KNO_3 and NaCl lies much below the point having the same osmotic value as the cell sap, on the assumption of purely osmotic action. Consequently NaCl and KNO_3 exert a pronounced toxic influence upon Spirogyra.

TYPES OF VEGETATION ON THE FLORIDA KEYS. By C. L. POLLARD, Washington, D. C.

THE observations recorded in this paper are based upon a trip of six weeks' duration, undertaken in the spring of the current year. Its primary purpose was the study and collection of the sub-tropical flora; but much attention was devoted to the ecological aspect of the flora, and an attempt is made in the paper to determine and differentiate six distinct belts or zones of plant life. A short enumeration and discussion of the characteristic species of these belts occupy the main portion of the paper, which concludes with certain inferences as to the future condition of the Florida Keys, judged in the light of present geographical conditions.

POTATO AS A CULTURE MEDIUM, WITH SOME NOTES ON A SYNTHESIZED SUBSTITUTE. By Dr. ERWIN F. SMITH, Washington, D. C.

AFTER discussing some of the advantages and disadvantages of potato as a substratum for the growth of fungi and bacteria, and expressing the belief, in opposition to Grimbart and some others, that it is a very useful substance and should not be discarded, the author proceeded to describe a synthesized medium which he had found free from some of the objections urged against the potato while possessing many of its good qualities. This medium the author designated as *nutrient starch jelly*. Its mode of preparation is as follows: 1. Dry potato starch 2 grams; this is put into a clean test tube and covered with 10cc. of the following fluid which should be shaken before measuring out, especially if it has been recently steamed.

2. Modified Uschinsky. This is made as follows:

	GRAMS.
Distilled water	1,000.000
Ammonium lactate	5.000
Sodium asparaginate	2.500
Sodium sulphate	2.500
Sodium chloride	2.500
Dipotassium phosphate (K_2HPO_4) . .	2.500
Calcium chloride	0.010
Magnesium sulphate	0.010

Instead of this fluid Uschinsky's solution may be used leaving out the glycerine.

In place of potato starch, rice starch or any other starch may be used. Owing to their usual alkaline reaction and to contaminating organisms in commercial starches it is best to prepare the starch one's self, washing the clean cloth-filtered starch repeatedly at short intervals, for a day or two in a large vessel in distilled water and drying quickly to avoid the multiplication of heat-resistant bacteria which will pass through the oven uninjured and subsequently spoil the medium. All of the salts used should be chemically pure.

After the starch has been thoroughly rubbed up in the fluid with a clean glass rod, each tube is slanted quickly in a dry oven or chamber such as is used for the coagulation of blood serum. From this point on the treatment is much the same as for blood serum; *i. e.*, exposure for two to three hours on each of five or six consecutive days to a temperature of not less than 75°C. nor more than 85°C. The starch swells and solidifies, at about 65°C. forming a smooth, moist, opalescent, bluish-white, solid medium which the subsequent heatings serve to sterilize provided resistant organisms have not been inadvertently introduced along with the starch. The cotton plugs should fit tight and after the second heating I usually find it necessary to remove the plugs and add 2cc. of distilled water to each tube to compensate for that driven off by the long exposure in the oven.

I have not been able to prepare this medium satisfactorily in the steamer, not even at a temperature of 97°C., owing to the fact that the slant sur-

face is ruptured and roughened by escaping vapor. A thin film of solidified starch usually forms on the wall of the tube over the slant and would prove very inconvenient did it not usually crack and fall off toward the close of the heatings in the dry oven.

This medium possesses excellent keeping qualities and is well adapted to the long-continued growth of many organisms both fungi and bacteria. It is particularly valuable for studying the diastatic action of various bacteria, some of which grow luxuriantly and convert all of the two grams of starch into sugar in five or six days, while others make a very feeble growth being able to convert the starch into carbohydrates available for their growth only with the greatest difficulty and in extremely small amounts.

This diastatic action is readily detected by the eye (change from bluish opalescent to opaque white); by the use of iodine (absence of starch reaction); by Fehling's solution (presence of sugar); and by shaking in water, the part acted on by the diastatic ferment dissolving readily into a white pap while that unacted on remains bluish opalescent, elastic and insoluble. Some organisms also appear to consume the starch *in toto* forming deep, dry pits. Inasmuch as there is great variability in the diastatic power of different micro-organisms, this test, now that it can be applied so conveniently and exactly, appears to be quite as useful for the differentiation of species as the liquefaction of gelatine and blood serum, or the production of gas. In my judgment it would be desirable to add it to the list of stock culture media on which all germs should be tried.

On adding assimilable sugars to this nutrient starch jelly, those organisms which previously made only a very feeble growth, *e. g.*, *Sarcina aurantiaca*, *B. coli-communis*, *Ps. hyacinthi*, now make a good growth. In other words, this substratum may also be used to test the nutrient value of various sugars, gums, alcohols, etc., whenever the organism we are experimenting with is unable or is able only very imperfectly to obtain its necessary carbohydrate material from the starch itself, *i. e.*, has little or no diastatic action. These substances may be used in any quantity desired and should be put in and dissolved before the starch is coagulated. I am in the habit of using 500 milligrams for each 10cc. of fluid, and have found the following substances useful, viz.: grape sugar, fruit sugar, cane sugar, milk sugar, galactose, maltose, dextrine, mannite, glycerine. Very likely the list might be profitably extended to include several other gums and alcohols.

Fifty tubes of this medium containing thirty species of bacteria, pathogenic and non-pathogenic, were passed about.

SOME LITTLE-USED CULTURE MEDIA WHICH HAVE PROVED VALUABLE FOR DIFFERENTIATION OF SPECIES. By Dr. ERWIN F. SMITH, Washington, D. C.

THE following were specially recommended:

(1) Uschinsky's solution.

(2) Tubes of standard nutrient agar with addition of 10, 20 and 30 % of grape sugar.

(3) The same with 10, 20 and 30 % of cane sugar.

(4) Nutrient starch jelly with and without various sugars, alcohols, etc.

(5) Cylinders of cocoanut flesh in distilled water.

(6) Cylinders of white or yellow turnips in distilled water.

(7) Cylinders of carrot in distilled water.

(8) Cylinders of white sugar beet in distilled water.

(9) Potato cylinders standing in several cubic centimeters of distilled water (for prolonged growth).

(10) The extension of tests with fermentation tubes so as to include also, in addition to the three sugars in common use, viz., grape, cane and milk, the following substances: fructose, galactose, maltose, dextrine, mannite and glycerine.

The crude vegetable substances named contain very different quantities and kinds of nutrient substances and consequently are very useful for bringing out hidden peculiarities of the germs which, when once discovered, may then be studied by more exact methods. To illustrate, cocoanut flesh contains little sugar and no starch, but is rich in oil. Potato flesh is poor in sugar, but contains several acids and is rich in starch. Sugar beet is free from starch but very rich in cane sugar. If now, we have an organism peculiarly fond of cane sugar, this fact is brought out by the very first test on sugar beet, whereas one might cultivate the organism indefinitely in beef broth, or on agar, gelatine or potato without discovering it. This is only one illustration of many. If we are to obtain an exact knowledge of these minute and multitudinous organisms we cannot experiment too widely.

THE TEMPERATURE AND TRANSPIRATION OF DESERT PLANTS. By D. T. MACDOUGAL, Minneapolis, Minn.

AN account of some experiments in the field in Arizona in 1898.

THE LEAF-SPOT DISEASE OF APPLE, PHYLLOSTICTA PIRINA SACC., AND SEVERAL UNRELATED FORMS OCCURRING THEREWITH. By WM. B. ALWOOD, Blacksburg, Va.

THIS paper records the common occurrence and life cycle of *Phyllosticta pirina* Sacc., and notes the occurrence therewith of three apparently unrelated forms, viz., *Sphæropsis malorum* Peck, *Hendersonia mali* and an undetermined species.

NOTES ON SOME DISEASES OF SOUTHERN PINES. By HERMANN VON SCHRENK, St. Louis, Mo.

OF the numerous parasitic fungi attacking the species of southern pines, *Trametes pini* is one of the most destructive. The same attacks trees over fifty years of age, causing disintegration of the heartwood. Only a part of the wood fibres is destroyed, being reduced to cellulose before their solution. The part not destroyed seems to be protected by some chemical substance, allied to the humus compounds, rendering it proof against the attacks of the fungus threads. *Trametes radiciperda* was found destroying numerous trees of *Pinus palustris* and *P. echinata*

A REMARKABLE INCREASE IN SIZE OF LEAVES OF KALMIA ANGUSTIFOLIA APPARENTLY DUE TO REDUCTION OF LIGHT. By W. J. BEAL, Agricultural College, Mich.

THE leaves of different plants of this species vary much in size, and considerably in shape, as every botanist may have observed.

Several botanists have recorded experiments showing that the leaves of dicotyledons grown in shade, or with the light reduced, are larger than those grown in full sunlight.

On May 14, 1898, before flowering, some plants with a little soil attached were removed from an open place at Grayling, Crawford County, Michigan, where they are common, and transferred to the State Agricultural College, 130 miles south, where they were planted in muck kept wet, and over the tops about eight feet above was a screen of lath and leaves of trees, which cut off at least half of the light during the middle of the day, and a larger per cent morning and evening.

On the 29th of July, the branches here exhibited were cut from the plants and pressed till dry. The largest leaf of this year on each of the three branches when dry is 37 x 17, 45 x 16, 47 x 15 mm. respectively, while the largest evergreen leaf of the previous year was 22 x 9, 25 x 6, 23 x 6 mm. respectively, indicating that a leaf of this year easily has a surface four times that of a leaf of last year. Little attention has yet been given to a comparison of the structure of these leaves, as those of this season are not yet mature. There is a possibility that part of this difference in size is due to transferring the plants to a better soil, though we should hardly look for such results on the first year.

At the same time and in the same place some plants of *Kalmia glauca* were transplanted. The largest leaves of this year in some cases have a surface double that of 1897.

HALF-SHADE AND VEGETATION. By BYRON D. HALSTED, New Brunswick, N. J.

EXPERIMENTS were made in shading various truck crops with frames of lath placed upon supporting stakes. The space between the lath equalled the width of the lath, so that a half of the direct rays were intercepted.

Daily observations of a thermometer in the shade and in the open showed that the temperature under the lath frames by monthly averages was lower than in the full sun as follows: May, 4°; June, 4.2°; July, 6.5°; August, 7.7°; September, 16.6°.

In general the shade retarded germination noticeably of lima beans for the first crop; but the opposite was true for the second or mid-summer planting.

All root crops, as turnips and carrots, including potatoes, had a larger leaf surface in the shade but the roots were much smaller.

The shade improved the salad crops, namely, lettuce, spinach and Swiss chard. The crop that was most improved of all by shade was celery.

With the seed of the fruit-producing crops as beans, peas, egg plants, tomatoes, cucumbers and the like, the time of blooming was retarded and period of fruitfulness materially prolonged. Wax beans, for example, at harvest had 103 green pods for the exposed to 287 for the shaded area.

The foliage is of a deeper green in the shade than in the open. In the carrots there is a strong tendency to broaden the dissected portions; in short, there is an attempt to increase the size of the blade in the various crop plants.

The port of the shaded plants is different from that in the sun. For example, the exposed bean plants in the brightest and warmest days made all sorts of shifts to reduce the exposure, changing the position of the leaflets from morning until evening, while in the shade the leaflets hung out horizontally and were not noticeably heliotropic.

The leaves in the shade were usually much thinner than those grown in full exposure. The record of this fact was made by sun prints of the leaves themselves, the shaded ones invariably being less opaque than the ones grown in the sun. Numerous sun prints were shown to establish the fact of the lesser density of the shade-grown leaves.

A study of wild plants along the same lines was made in a woodlot and adjoining clearing. The cinnamon fern (Osmunda) was noted in particular as having graceful, drooping leaves in the wooded land, while in the open they were nearly upright. Actual measurements of the thickness of the leaflets were made, those in the shade being .098 millimeter, while in the cleared land the thickness was .258 millimeter.

Many other differences as to hairiness, color of stems, etc., were noted between the shaded plants and those in the open.

INFLUENCE OF WET WEATHER UPON PARASITIC FUNGI. By BYRON D. HALSTED, New Brunswick, N. J.

THERE is some connection between the weather and the development of parasitic fungi. May of the present year was remarkably wet and the following June brought an unusual abundance of parasitic fungi. Among these were the rust of hollyhock, cedar and mandrake; smut of spring lily, wheat and oats, and many others. The peach leaf curl fungus was common everywhere.

The year 1889 was noted for its wet summer and also for the destruction of potatoes by the rot. The year nearest like 1889 since then was 1897 when again July was phenomenal for the heavy precipitation, it being 10.19 inches in 1889 and 11.42 in 1897. Last season was like that of eight years before in the outbreak of the Phytophthora in the potato fields causing the wholesale rotting of the tubers. The *Phytophthora phaseoli* Thax., was discovered in 1889 and reappeared destructively last season.

The records of the experiment station show that in both of the years mentioned there was much black rot (*Physalospora bidwellii* Ell.) of the grapes, and rust (*Gymnosporangium macropus* Lk.) in the apple orchards. There is, in short, a striking parallelism between the rainfall and the abundance of fungi.

In 1894 the growing season opened with a wet May and closed with a rainy September. There were long periods of cloudy weather and often it was hot and showery. It was this year that witnessed the unprecedented outbreak of the fire blight of apple, quince and pear.

In 1896 there was a rainy June and July and during this period the asparagus rust (*Puccinia asparagi* DC.) made its appearance in the Eastern United States to an alarming extent, and last year it was in even greater abundance.

There are not sufficient data for safe and substantial generalizations, but heavy rainfalls in spring seem to induce rusts in grains and orchard and garden crops, and the fire blights of fruit trees. Midsummer precipitations favor the decay of fruits, particularly the stone fruits, and the rotting of potatoes.

Rainfall does not express all the meteorological conditions that need to be taken into consideration in this connection.

THE BOTANIC GARDENS AT BUITENZORG, JAVA. By DAVID G. FAIRCHILD, Washington, D. C.

A POPULAR lecture illustrated by forty lantern slides, showing buildings, grounds, and characteristic vegetation of the Dutch gardens at Buitenzorg and Tijdbodas.

STRAND FLORA OF FLORIDA. By HERBERT J. WEBBER, Washington, D. C.

A POPULAR lecture illustrated by fifty lantern slides, showing how the coast of Florida has been extended into the sea through the combined action of the waves and of sand-arresting and sand-binding plants.

NOTES ON THE RELATIVE INFREQUENCY OF FUNGI UPON THE TRANS-MISSOURI PLAINS AND THE ADJACENT FOOTHILLS OF THE ROCKY MOUNTAINS. By CHARLES E. BESSEY, Lincoln, Neb.

FERMENTATION WITHOUT LIVE CELLS. By KATHERINE E. GOLDEN and CARLETON G. FERRIS, Lafayette, Ind.

IN our experiments we followed Büchner's method in regard to the preparation of the yeast. In filtering we first used three thicknesses of the ordinary filter paper, filtering twice, then two thicknesses of parchment paper; but, upon microscopic examination of the extract, live cells were found, although the extract appeared clear and opalescent, agreeing apparently with the description of Büchner's. A filter was then made of gypsum. This was made by putting two thicknesses of filter paper in a glass funnel, then coating the paper with about one-half inch of gypsum. The filter was then fitted into a flask and the whole sterilized. The filtrate from this was clear, but also contained live cells, though few in number. The filtrate remained clear for three days, then became murky from the growth of yeast and bacteria. A similar filter, but having the gypsum an inch thick, was tested with like results. In the experiments which followed, a porous cup was used and, as the liquid filtered very slowly, the apparatus was connected with an aspirator. With this filter we were able to obtain a sterile extract. The extract was tested in ten and twenty per cent cane sugar, dextrose, and wort solutions, but no fermentation took place in any of them. This was tested at three separate times and at 37½° C. as well as at the room temperature, but with negative results in every case.

The yeast was then changed to another compressed yeast, which also gives vigorous fermentation, but in no case was any fermentation obtained. Will, in his criticism of Büchner's work, states that in his judgment Büchner's yeast was in a state of decomposition. With this criticism in mind we kept quantities of both compressed yeasts in a moist condition until they gave off a rank odor, then dried them and proceeded as with the fresh ones, testing the extract in ten and twenty per cent solutions of the various sugars, but with similar negative results to those obtained with the fresh yeast extracts.

SALT AS A DETERRENT IN YEAST FERMENTATION. By KATHARINE E. GOLDEN, Lafayette, Ind.

IN the fermentation of bread, salt is always one of the ingredients, not used, however, for any effect it has on the fermentation, but merely as a flavoring. To determine the effect the salt has beyond the flavoring, some experiments were carried out, using sponges, doughs, and lastly solutions containing no flour.

In the first set of experiments six different sponges were made, containing equal quantities of flour, water, yeast, and salt, varying in quantity from one per cent to five per cent of the water used. One sponge had no salt added; this to be used as a control. In mixing the sponges the salt and yeast were added to the water before the flour so as to ensure a thorough mixing. The sponges were placed in an oven at 23° C., and observations taken at intervals of half an hour. A second series was conducted at 37° C. and a third at 40° C. For ease in reading slight variations in the fermentation, the sponges were mixed in long test tubes of nearly uniform diameter. In the corresponding numbers of the three series the fermentation action was fairly uniform, that is, there were practically the same differences between 1% and 2% whether fermented at 23° C. or 40° C. The following table shows the average of the three series, the figures showing much regularity in the action. As the quantity of salt increased from 1% up, the action of fermentation was checked in a corresponding ratio. These sets of experiments were repeated three times, and showed very slight variations in the results, so that the set of figures given are typical of the action.

		INCREMENTS OF FERMENTATION IN INCHES.			
EXP.	% SALT.	½ HOUR.	1 HOUR.	1½ HOURS.	2 HOURS.
a	0	.666	1.375	1.708	1.583
b	1	.542	1.292	1.625	1.625
c	2	.456	1.200	1.500	1.666
d	3	.354	.958	1.375	1.375
e	4	.313	.875	1.250	1.375
f	5	.146	.292	.438	1.666

A series of experiments with stiff doughs was then conducted; and in the doughs, as in the sponges, the salt acted as a deterrent, though the slight variations that could be seen easily in the sponges could not be detected in the doughs as broader and firmer vessels had to be used for the mixing.

In these experiments where flour entered as an ingredient, it would be impossible to determine whether the yeast was acted upon directly by

the salt, or indirectly by the action of the salt upon some constituent of the flour required by the yeast. To obviate this difficulty in interpretation a series of experiments was made, using Pasteur solution with cane sugar. These were carried on in fermentation tubes. As in the former experiments, equal quantities of the solution and of the yeast were used in each. The salt varied from one to four grams, or 4, 8, 12, and 16% respectively of the solution used as the tubes held 25 cc. solution. The control formed sufficient gas to displace the solution in the tube in 23 hours, the 4% tube took 37 hours, 8% took 38 hours, 12% 47 hours, while the 16% took 7 days.

The fermentation in this series was weak, due probably to the yeast which was from a dry yeast cake, and though there was a gram used in each tube, this gives only a small quantity of yeast, as the cake contains much corn meal. In the next series a compressed yeast was used, one-half gram in each tube. The action of fermentation was more vigorous and more regular than in the former series. The observations were taken every quarter hour.

EXP.	% SALT.	VOLUME OF GAS IN CC. HOURS:— ¼	½	¾	1	1¼	1½	1¾	2	2½	3	7	20	23
a	0	3	2	3	2½	3	3	2½	6	.....	.....	.....	.....	.....
b	2	4	2	1½	2	2½	2	2	6	3	.....	.....	.....	.....
c	4	1	1	1½	1	1½	1	1½	4	4	2	6½	.....	.....
d	6	.....	.....	.....	.....	1	⅔	⅔	2½	1½	1½	15½	2	.....
e	8	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	6	12½	2½

The figures in the table are the number of cc. of gas generated in the given periods of time, and show clearly that, as the quantity of salt increased, the vigor of fermentation decreased.

In the next series of experiments the quantities of the yeast and the solution were the same as in the preceding, but the quantity of salt was decreased; this in order to determine how small a quantity could be used and still get a deterrent action, 1, 2, 3 and 4 decigrams of salt, or .4%, .8%, 1.2%, 1.6% respectively of the solution were used. In this set the control started fermentation but very slightly ahead of those containing the salt and then kept that same amount ahead, but there was no appreciable difference between any of the others.

The experiments indicate that salt has a deterrent action upon fermentation, even when used in very small quantities, and that after a certain quantity of salt is used, the action is in a direct ratio with the quantity of salt used; also that the action is directly upon the yeast, and does not depend upon other ingredients of the sponge.

Boutroux[1] made some experiments upon leaven, using tartaric acid. In

[1] Boutroux, L. Le Pain et la Panification, 1897.

France a different method from ours for fermenting the bread is used. There a portion of the dough is kept from one baking to the next to be mixed with the fresh dough. In Boutroux's experiments he used the tartaric acid in the leaven and in the dough, and found that the dough fermented more vigorously when the acid was in the fresh dough than when in the leaven. He then tried leaven in acid and neutral wort, and obtained vigorous growth in the neutral wort, and but slight growth in the acid wort. He states that salt in dough should act like the tartaric acid, that it retards the fermentation when it is used in any but very small quantities.

Jago[1] found that salt retarded the fermentation of the dough, but he found no difference in the action between a 1.4 % salt solution and a 2.9 %; but found that a 5 % solution retarded the fermentation very materially. He advises using the salt in the last kneading. This is practicable where a sponge is first made, then the dough; but many persons make a stiff dough at once, and if the salt were kept for the last kneading, it could not be mixed thoroughly with the dough, but would be more liable to stay in pockets. The experiments indicate that where a sponge is used and a quick fermentation desired, the salt would better be added in the last stages.

FUNGUS GARDENING AS PRACTISED BY THE TERMITES IN WEST AFRICA AND JAVA. By O. F. COOK and D. G. FAIRCHILD, Washington, D. C.

THIS paper describes the nests of termites found in Africa and Java, and the fungi taken therefrom. These fungi are cultivated by the termites for food in the same way as is done by ants (species of Atta) in Central and South America. The paper was illustrated by numerous photographs showing sections of the nests with the fungi in place, the little swollen hyphal tips used for food, etc. Some of the nests are very large.

THE BIOLOGY OF CHEESE RIPENING. By Profs. S. M. BABCOCK and H. L. RUSSELL, Madison, Wis.

THE most important changes which occur during the ripening of cheese are those which affect the casein, this being gradually transformed from the firm, elastic, and insoluble condition found in a green cheese into the plastic, and more or less soluble substance peculiar to a well-ripened product. The early explanations of these changes were purely chemical; but since the discoveries of Pasteur and others in the field of fermentation they have been attributed entirely to bacteria and other micro-organ-

[1] Jago, W. Science and art of bread-making, 1895.

isms. This theory is made very plausible by the enormous development of bacteria in cheese and the fact that strong antiseptics which inhibit bacterial growth also prevent the normal changes. Duclaux advanced the hypothesis that the changes in the casein were caused by digesting organisms, on account of the similar nature of the products formed. Later observers however have shown that such organisms fail to develop in competition with the lactic acid type of bacteria, which are by far the most prominent species found in normal cheese. On the other hand the lactic acid type of bacteria appear to be unable to digest casein to any considerable extent when grown in sterilized milk, unless their activity is greatly prolonged by neutralizing the acid as it is formed, as has been done by Freudenreich, in which case, again, the conditions do not conform to those found in normal cheese. Moreover the ripening changes in cheese progress at a nearly uniform rate for a long time after bacterial development has greatly declined.

The authors of this paper were unable to reconcile the many apparent discrepancies of the biological theory until they attempted to sterilize milk for their experiments by the addition of mild antiseptics, such as ether and chloroform, which could afterwards be removed, and thus avoid any changes which might be produced by boiling the milk. Such milks although sterile underwent similar changes to those that occur in cheese. As the agents used in this case discriminate between organized and unorganized ferments, it is evident that milk contains an unorganized ferment capable of digesting casein. They have also shown that this enzyme is inherent in the milk itself and have succeeded in isolating the same from milk and producing all of the changes that normally occur in the raw milk. The authors have given to this ferment the name GALACTASE and they believe that it plays an important rôle in the proteolytic changes that occur in the ripening of cheese.

THE LEAVES OF THE RED ASTRACHAN APPLE IMMUNE FROM THE ATTACK OF GYMNOSPORANGIUM MACROPUS. By W. J. BEAL, Agricultural College, Mich.

WISHING to cultivate this parasite in the botanic garden and as it was not to be found on the red cedars of central Michigan, my friend, Dr. Halsted, of New Jersey, induced some one there to send me half a dozen young cedars, well supplied with living galls of Gymnosporangium. This was early in the spring of 1897. Near the cedars, almost in contact, I set two young apple trees of the variety red astrachan. While the leaves of the apple were yet small, I applied artificially in three places some teleutospores from the cedar. Several times afterwards, during the spring and summer, examinations were made, but no traces of the fungus could be found on the leaves of the apple. The tests were repeated in 1898, with the same results. A letter from Prof. L. R. Jones, of Vermont, states that he has tried to induce the teleutospores from the red cedar to attack red astrachans with negative results.

On the occurrence of a yeast form in the life cycle of the black rot of apple, Sphaeropsis malorum, Peck. By Wm. B. Alwood, Blacksburg, Va.

This paper records the discovery of a yeast form occurring in laboratory cultures of this fungus which on isolation and reinfection on the fruit of apple produced the common fruiting bodies characteristic of *S. malorum*.

Notes on Stewart's sweet-corn germ, Pseudomonas stewarti, n. sp. By Dr. Erwin F. Smith, Washington, D. C.

In the early part of 1898 I received from Mr. Stewart a pure culture of this organism with the request that I should work it over and name it. Since that time I have carried it through many cultures, comparing it with other organisms. It seems to be an undescribed form. It is motile by means of a polar flagellum and I have consequently designated it *Pseudomonas stewarti*, in recognition of Mr. Stewart who first called attention to it. With some exceptions noted below it possesses, so far as I have studied it, the characters assigned to it by Mr. Stewart[1] and some others which will help to distinguish it. The question of its parasitic nature must be left where Mr. Stewart left it. I consider that he has not proved it to be a parasite, although he has certainly rendered it probable.[2]

The particulars in which I have been unable to agree with Mr. Stewart are the following:

STEWART	SMITH.
Color of agar at first a *[illegible]* white changing to a dirty yellow.	Buff from the start.
Or rather the "organism grows very rapidly."	grows slowly and feebly on potato in comparison with many other germs, e. g., *Bacillus oleaceus*, *Ps. campestris*, *Ps. zeæ*, etc. The *Ps. stewarti* makes fifty to one hundred times as much growth on potato.
It takes a month to produce white [illegible].	A buff precipitate.
It does not [illegible] the potato. [illegible]	Potato becomes *gray*. It is not [illegible].

[1] Stewart, Freeman, and Sherwood, Bull. No. 78, Cornell Univ. Agr. Exp. Sta., p. 5.

[2] Since the above was written, Mr. Stewart has informed me that he has found in Southwestern Michigan [illegible]. These worms were fatty or [illegible]. In this yellow organism, which [illegible] Mr. Stewart's account is surprising in so

STEWART.	SMITH.
The quantity [of alkali] is not sufficient to overcome the slight acidity of the water in the bottom of the tubes containing sterilized potato cylinders. And the behavior of tubes of litmus milk inoculated with the germ indicates the production of acid rather than alkali.	In six days the fluid in my potato cultures changed from feebly acid to feebly alkaline and became strongly alkaline in three weeks to neutral litmus. The behavior in litmus milk, as described by Mr. Stewart, is simply that of many bacteria which reduce litmus to a colorless substance without the production of any acid.
In beef bouillon at the end of three weeks some flocculent white precipitate is formed.	In 1:2 beef broth made strongly alkaline to litmus (neutral to phenolphthalein) by adding caustic soda the precipitate was bright buff; in acid beef broth (unneutralized) it was dark buff.
Organism appears to grow best on neutral or slightly acid media. A comparatively slight degree of alkalinity is sufficient to prevent growth, but on the acid side there is a wider range.	Organism grows much better in beef broth neutral to phenolphthalein (strongly alkaline to litmus) than in acid beef broth (1:2 unneutralized). This germ grew well in beef broth neutral or even slightly alkaline to phenolphthalein. It is, however, not very sensitive to certain acids.

ADDITIONAL DATA.

(1) The organism grows readily on Loeffler's solidified blood serum, forming a bright buff layer dense enough to hide the substratum. It does not induce any liquefaction (four weeks). In this particular it differs from *Ps. campestris* and a number of other one-flagellate germs known to the writer.

(2) The organism grows best in the presence of cane sugar. Cultures on potato or turnip cylinders, which contain but little sugar, when placed side by side with cultures on cylinders of sugar beets, show a most surprising difference. The growth is ten to fifty times as abundant on the sugar beet. The same differences were obtained in bouillon and on agar by adding or withholding cane sugar. If the organism is a parasite, we have in this fondness for sugar an explanation of why sweet corn is attacked in preference to common corn. Some other sugars also stimulate growth, *e. g.*, grape sugar and galactose. In 5% doses, dextrine, mannite, glycerine, lactose and maltose have no favorable influence. Possibly 5% glycerine has a restraining influence. The germ grows readily on nutrient starch jelly in presence of 5% lactose or 5% mannite when another organism is present to convert starch for it.

be a parasite. Often at least infection takes place in early stages of growth and almost certainly *through the water pores at the apex of the leaf.* I saw cases where the infection had proceeded part way down a leaf, and cutting off the upper five to ten mm. of the shrivelled tip and mounting in water the bacteria swarmed out of the cut bundles and also from the extreme tip of the leaf and from no other parts. Several times two shoots from one root were seen with only one of the two plants affected.

(3) The organism makes a very copious and surprisingly long-continued bright buff growth in Uschinsky's solution. This is the most suitable nutrient fluid I have found. It is better by far than beef broth or potato broth. For comparison half a dozen other yellow germs were grown in this fluid, but none of them showed anything like as much vigor. For instance this germ gave a much more prolonged cloudiness and ten to fifteen times as much precipitate as *Ps. campestris*, *Ps. phaseoli* or *Ps. hyacinthi*. The fluid clouded in forty-eight hours and was very turbid and fully alive at the end of three months. At this time the fluid was intensely alkaline and the color of the slime (placed on white paper and compared with Ridgway's tables) was between his ochre yellow and chrome yellow.

(4) The diastatic action appears to be rather feeble. On my nutrient starch jelly (Uschinsky's solution with starch in place of glycerine) the growth was rather feeble, resembling in this respect *Ps. hyacinthi*, and at the end of two months there was a strong iodine starch reaction immediately under the germ layer. There was also a strong starch reaction immediately under the germ layer on potato after seventy-three days' growth. Such cultures were strongly alkaline and had a disagreeable smell, common to many bacteria and a little suggestive of soured cooked potatoes, but the cylinders were not softened. When cane sugar or other nutrient sugars were added the growth proceeded rapidly and became abundant, but the diastatic action was not much increased. At the end of twenty-four days the growth on the nutrient starch jelly with 5 % cane sugar was estimated to be one hundred times as great as on the same medium without the sugar.

(5) Organism grows moderately well on carrot, not always obscuring the color of the substratum. It grows better than on turnip or potato, probably because it contains more sugar, but not nearly so well as on sugar beet.

(6) On cocoanut flesh after two weeks at 20° to 27° C., the growth was bright buff and sufficient to hide the substratum. In the fluid there was about the same amount of precipitate as in the tubes of carrot. At the end of eleven weeks the fluid was strongly alkaline.

(7) Acid beef broth (1 :2 unneutralized) retarded growth. At the close of the third week the fluid was strongly alkaline to litmus.

(8) Strongly acid beef broth (1 :2 concentrated by boiling,—acidity of 1cc. $=0.80$ cc. $\frac{N}{10}$ NaOH) strongly retarded growth and prevented it altogether if only a few germs were put into the tubes.

(9) The organism grew well in half-strength potato broth (1 :4) with the addition of a small amount of malic acid (acidity of 1cc. of broth $=0.45$ cc. $\frac{N}{10}$ NaOH). It is much less sensitive to malic acid than *Ps. hyacinthi* or *Ps. campestris*.

(10) In the potato broth without addition of malic acid (acidity of 1cc.= 0.30 cc. $\frac{N}{10}$ NaOH) the behavior of the organism was much the same. The clouding did not occur sooner and was not heavier. The striking feature of the potato broths, both with and without the malic acid, was the absence of any bright color either in the precipitate, the fluid itself, or the mass of germs forming a rim at its surface. The deepest pigmentation was only a dirty yellowish white. This was in marked contrast to the color in beef broth, or in Uschinsky's solution.

(11) On the slant surface of standard nutrient agar (1 litre distilled water, 10 gr. agar, 2.5 gr. Liebig's beef extract, 10 gr. Witte's peptone, neutralized with sodium carbonate) the growth was rather feeble. The slime was strongly alkaline and its color on old cultures was between buff-yellow and chrome-yellow (Ridgway). This agar was + 15.5 Fuller's scale and was free from muscle sugar. The growth of the germs on the same agar with the addition of 1 gram of c. p. grape sugar for each 10cc. of agar was *ten times as abundant.*

(12) In the cooked juice of green cabbage leaves undiluted with water (acidity of 1cc. = 0.40 cc. $\frac{N}{10}$ NaOH) this organism grew without retardation, *i. e.*, the fluid became clouded in forty-eight hours. It remained acid for the first seven days but was only feebly so on the seventh day (litmus). It was feebly alkaline on the fourteenth day, and strongly so at the end of four weeks, remaining so. There was a copious dirty buff precipitate. Five other species of bacteria parasitic to plants were inoculated into this same medium but none of them would grow. One of these was *Ps. campestris.*

(13) The organism was inoculated into four different, cooked, acid tomato juices (Nos. 334, 333, 332 and 331, undiluted by water) viz.: from small green fruits, from full-grown green fruits, from fruits just beginning to flush and from fully ripe (red) fruits. These were carefully titrated for acidity and sugar content and were quite acid, *i. e.* 50% more so than the cabbage juice. Development was retarded at first but in three of the broths the organism overcame the hindering substances and made a good growth after the first week, most in those stocks which titration had shown to contain the most sugar. Stock No. 331 was still strongly acid at end of thirty days, although the organism had made such an excellent growth in it that it was barely possible to see the penholder behind the tube. In No. 332, no growth could be obtained, although it contained more sugar than 331 and was of about the same grade of acidity. This was attributed to the presence of an inhibiting acid different from that of the ripe fruit. Six other bacteria parasitic to plants were inoculated into these broths (very copiously) but none of them grew.

(14) At the end of forty-seven days the slime in 10 cc. of agar with 1 gram of grape sugar was distinctly acid, while in the same agar without the sugar the slime was strongly alkaline to litmus. At the end of eighty-six days the slime in a sugar beet culture showed no alkaline reaction, but dried acid on neutral litmus paper, while the slime from corresponding cultures on potato and cocoanut, although less abundant, was strongly alkaline. There can be no doubt, therefore, that one or more acids are produced by the growth of the germ in the presence of assimilable sugars, not, however, out of lactose.

(15) No gas was produced in any of my cultures.

(16) No spores have been seen.

SYNOPSIS OF CHARACTERS.

A medium-sized rod rounded at the ends and motile by means of one polar flagellum, size 0.5 to 0.9 by 1 to 2μ, no spores observed; found in enormous numbers in the vascular bundles of corn (*Zea mays*) associated with a destructive disease of which it is probably the cause; color in the host plant and in culture media yellow (buff to chrome or ochre, occasionally a pale dirty yellow); aërobic and facultative anaërobic; grows in all ordinary culture media; bears alkali well (soda) and plant acids extremely well; grows luxuriantly in Uschinsky's solution; growth enormously stimulated by cane sugar, grape sugar, and galactose; growth not favored by 5% doses of lactose, maltose, dextrine, mannite, or glycerine in nutrient starch jelly; diastatic action feeble, *i. e.* able to obtain food from starch only with much difficulty; produces alkalies in all sorts of media and acids in the presence of grape and cane sugar; reduces litmus slowly; does not liquefy gelatin (Stewart); does not liquefy Loeffler's blood serum; grows well at summer temperatures of 25° to 30°C; does not die out quickly in culture media; does not produce gas; sensitive to light (Stewart); occurs in New York and Michigan and may be looked for in all parts of the United States.

A BACTERIOLOGICAL STUDY OF PEAR BLIGHT. By LILLIAN SNYDER, Lafayette, Ind.

IN connection with the true pear blight bacillus, *Micrococcus amylovorus*, Bur., another germ is found. This second germ together with the blight bacillus has been carried through a number of experiments, including inoculations into the tree in order to compare the two germs and to determine if they are related to each other. In these experiments it is found that they are very similar morphologically but different in physiological characters. The second germ cannot be isolated from the interior of the diseased portion unless the needle (if the platinum needle is used to transfer the germs) comes in contact with the surface of the

branches from which the cultures are taken. This germ may be obtained by putting pieces of diseased tissue in nutrient solution. When healthy tissue is placed in nutrient solution the germ is also obtained, or when the surface of healthy tissue is washed in a nutrient solution this germ appears. This together with other experiments has led to the conclusion that the second germ isolated from the tree in connection with the blight germ is only a surface germ and not really parasitic.

THE LIFE-HISTORY AND CHARACTERISTICS OF THE PEAR-BLIGHT GERM. By M. B. WAITE, Washington, D. C.

BEGINNING in the spring the germs on the new growth of the season first appear on the nectar disks of the blossoms. The bacilli live and multiply in the nectar and are able to enter the nectar glands without a puncture or injury and thus normally get inside their host. The distribution from flower to flower and tree to tree is through the agency of insects, mainly flower-visiting sorts. Infection also occurs on the young shoots and less frequently on the fleshy bark through injuries. Insects and birds are agents of distribution and inoculation in these cases. No evidence could be found that the germs are carried by the wind. The blight germs usually die out in the twigs which are blighted and dead but in certain cases the germs manage to keep alive through the summer by making slow progress in the fleshy bark. Such cases may succeed in living over winter. Winter weather is favorable to the longevity on account of the moisture and low temperature. The cases of "hold over" blight start off again in spring and exude quantities of gummy matter full of the bacilli. This is visited by insects, especially flies and wasps, and carried on to the newly-opened flowers thus completing the life cycle.

The condensed characters may be given as follows: An oval rod like bacillus .6 – .8 μ by 1 to 6 μ long. Constant in diameter but varying greatly in length. Occurs singly or in young cultures in pairs, chains or masses. Stains readily with the ordinary aniline dyes either watery or alcoholic solutions. Has no capsule, but is supplied with several flagella scattered over the surface. It is actively motile. Does not produce spores. On nutrient beef and potato broth produces first a strong turbidity and a slight granular pellicle on the surface, which breaks up and settles to the bottom. The color of the mass is milky white on all solid media.

On agar plates the outside colonies at ordinary temperatures 18-20° C. reach a diameter of about 1 mm. in forty-eight hours and at the end of a week become 5 to 6 mm. across. A temperature of 36 – 37° C. starts the growth more promptly but results in a feebler ultimate development.

The addition of malic or citric acids in small amounts so as to acidify the agar increases the vigor of growth while an excess of alkali dimin-

ishes it. On gelatine made from the commercial brands, the opposite effect is produced. Gelatine should be neutral to phænolphthalein to insure vigorous development. There is a moderate liquefaction in good gelatine cultures. A moderate growth is made on sterile potato cylinders.

In the fermentation tube it decomposes sugar without the formation of gas. It is most vigorous on maltose the cultures becoming strongly acid, and is slightly less so on cane sugar, dextrose and levulose. It is aërobic and facultative anaërobic. It produces no pigment or coloring matter of any sort and no odor. It does not decompose starch. Its principal food consists of nitrogenous matter, sugars and probably, to some extent, certain organic acids, the very substances which occur in vigorous, young, growing tissues of the host. Certain statements formerly made are now known to be erroneous.

The germ mass is said to be yellowish white on potato. This could only come from an impure culture as the true pear-blight germ is always white. Gas, or in some places CO_2 gas, is said to be formed. This never occurs. Butyric acid is said to be one of the products of its decomposition. The germ produces acids but never butyric. Starch is said to be decomposed and used as a food, but so far we have never been able to demonstrate this. The germ is said to live over winter in the soil. In our search we have failed to find it in such places and its life cycle is complete without it.

THE EFFECT OF SOME CHEMICAL FERTILIZERS UPON THE GERMINATION OF SEEDS. By GILBERT H. HICKS, Washington, D. C.

RÉSUMÉ of previous investigations, especially those of Tautphœus, Vilmorin, Hindorf, Nessler, Jarius, Claudel, and Crochetelle and Buffum.

In the progress of germination fertilizers may influence:

1. The swelling stage.
2. The nature and availability of the reserve materials.
3. The awakening and growth of the embryo.
4. The young sprout before it reaches the surface of the ground.

This paper attempts to show that the principal effect is in the latter instance.

Method of conducting the experiments upon which this article is based:

I. Account of tests made with seeds of wheat.
II. " " " " " " " lettuce.
III. " " " " " " " radish.
IV. " " " " " " " crimson clover.

CONCLUSIONS.

1. One per cent strengths of muriate of potash and sodium nitrate used as fertilizers are very detrimental to the germination of seeds, whether applied directly or mixed with the soil.

2. Fertilizers composed of phosphoric acid or lime are much less injurious to germination than sodium nitrate or muriate of potash, and if not used in excess may be harmless.

3. Commercial fertilizers should not be brought into direct contact with germinating seeds.

4. The effect of treating seeds with chemicals before planting is no index to the action of those chemicals when applied as manures to the soil.

5. The chief injury from chemical fertilizers is effected upon the young sprouts after they leave the seed coat and before they emerge from the soil, while the seeds themselves are injured only slightly or not at all.

6. It is highly improbable that potash, phosphoric acid, nitrogen or lime used as fertilizers actually favors germination.

DEVELOPMENT OF THE POLLEN GRAIN IN SYMPLOCARPUS AND PELTANDRA.

By B. M. DUGGAR, Ithaca, N. Y.

IN these studies all material was fixed in the Flemming chrom-osmo-acetic mixture, and stained mostly in parallel series with the Flemming triple stain and with iron hæmatoxylin and orange.

Division in the primitive archesporium is of the vegetative type, and the number of chromosomes present is that of the whole number of the sporophyte.

The resting nucleus of the definitive archesporium has a large nucleolus taking the chromatin stain in the Flemming combination. The reticulum is a loose net very slightly chromatic. In the cytoplasm there is no differentiated zone of kinoplasm.

Synapsis, or the contracted state of the chromatin thread in the late reticulum or early spirem, was found abundantly at a definite period in the life history of these cells prior to actual division. In this condition staining is difficult, and the return from this phase is characterized, especially, by a loosening of the ribbon into one perfect coil, thus truly initiating the spirem.

The spirem ribbon becomes nodulate, and finally segmentation is preceded by a bending back of the ribbon at definite points and by the disappearance of the chromatin along the connecting linin.

The formation of the spindle is multipolar, and the chromosomes are centrally as well as peripherally arranged.

The nucleolus is peculiar in assuming various shapes during segmentation and in showing linin connections with the general ribbon.

The first division, in general, indicates that there is a longitudinal division of the chromosomes, although in Symplocarpus there is a suggestion that the first division may be the reducing division, and hence transverse.

In the second division the daughter segments separate longitudinally in both cases. There is no return of the nucleolus prior to the second division

but a true dispirem is formed. This is in accord with studies on Liliaceæ, but differs from some dicotyledonous plants studied.

In the division of the microspore nucleus, the nucleus first migrates to one side of the cell, and the entering kinoplasm forms a multipolar, somewhat barrel-shaped spindle. This finally becomes completely attached at one pole, forming a truncated cone, while the other pole of the spindle may be truly conical. This fixity of the spindle causes the unequal division of the cell body, necessitating the small generative cell, and this method of division (fixity of one pole) seems to be characteristic of such divisions in many plants.

THE EMBRYOLOGY OF TAXUS. By E. J. DURAND, Ithaca, N. Y.

IN this paper the development of the female prothallium is traced from one cell of an initial axial row of about three cells. The nuclei which result from the division of that of the macrospore arrange themselves in a peripheral layer, and walls are formed between them so that the young prothallium is in the form of a hollow sphere. The hollow center gradually becomes solid from the ingrowth of the cells.

The archegonia are developed at the distal end of the prothallium. The neck consists of four cells, instead of one as is usually stated for this plant.

NOTES ON THE EMBRYO SACS OF CERTAIN MONOCOTYLEDONS. By K. M. WIEGAND, Ithaca, N. Y.

RECENT investigations made by the speaker tend to show that the two extreme types of embryo-sac formation, as illustrated by Lilium and Canna, are related in a manner not before observed. In Convallaria which represents the transitional type, a septum is formed after the first division of the hypodermal nucleus but not after the second. This represents an axial row of four cells with two septa omitted. The remaining septum at length breaks down so that a single cavity containing eight nuclei results. The single cell of Lilium is therefore derived from the four axial cells of Canna, not primarily through the omission of any divisions of the mother-cell but by the absence of the septa.

RAPIDITY OF CIRCUMNUTATION MOVEMENTS IN RELATION TO TEMPERATURE. By F. SIMONS and K. F. R. MCKENNEY, University of Pennsylvania.

Records are given for [illegible] species. Plants used were *Helianthus*, *Humulus*, [illegible], and *Helianthus annuus*.

Time of shortest circumnutation given, but few exact details as to temperature. Average [illegible] spring, 20°–23° C. in sun and

Various results already obtained at the Botanical Laboratory of University of Pennsylvania, pointed to likelihood of higher average temperature of Philadelphia giving more rapid results. Proved to be true for Dionæa, Mimosa, and hop.

In this paper no account is taken of relative light intensity, though data are being gathered which prove that this is an extremely important factor.

On dull cold days with temperature at 15°–19°C., movements were found to be *extremely* slow. The average optimum for best results was 28°C.

In *Convolvulus sepium* two distinct types of stem were observed: a rapidly circumnutating one, and a prostrate one showing extremely feeble movements.

Following are shortest periods as given by Darwin and as obtained by the writers.

DARWIN.		WRITERS.	
Phaseolus vulgaris, May 2,	1h 55m	April 5,	1h 20m
		July 18,	1h
Humulus lupulus, " hot weather,"	1h 8m	Feb. 9,	1h 5m
Convolvulus sepium,	1h 42m	March 25,	57m
Lonicera brachypoda, Apr. 3,	7h 30m	April 5,	2h 48m
		August 15,	1h 43m
Wistaria sinensis, May 16,	2h 5m	July 11,	2h

In all the above results obtained by the writers it is safe to assume that the temperature was on the average 12°C. higher than that worked in by Darwin. At the present stage of our investigations it would be rash to say that the higher temperature is the sole or even main determining factor. Light intensity and hygrometric conditions of the atmosphere have been found to coöperate also, but present indications are that temperature is a very important factor, and that an optimum as well as a maximum and minimum temperature exists for each species.

GENERAL CHARACTER OF THE DUNE FLORA OF SOUTHEASTERN VIRGINIA. By THOS. H. KEARNEY, Jr.

VEGETATION OF THE WOODED, FRESH-WATER SWAMPS OF SOUTHEASTERN VIRGINIA. By THOS. H. KEARNEY, Jr., Washington, D. C.

THESE two papers are preliminary studies towards a survey of the ecological distribution of vegetation in the coastal plain region of Virginia. The dunes and the large wooded swamps are selected, because in these two environments life conditions are most extreme, and plants are most obviously modified with relation thereto.

The scheme of the two papers is the same. First, topography, soils, heat, light and other physical conditions are described. Then follows a

sketch of the vegetation, with especial reference to the more or less sharply defined associations into which it is grouped. A brief synopsis of the life conditions prefaces an enumeration of the more evident adaptations by which the plants are brought into harmony with these conditions; and brief descriptions of the more important life forms. Finally, matters of general interest, not properly referable to any of the preceding subjects, receive brief mention.

ON THE VALIDITY OF THE GENERA SENNA AND CHAMÆCRISTA. By C. L. POLLARD, Washington, D. C.

This is a record of further observations on the structure of the flower of *Cassia chamæcrista*, on the floral arrangement of which Professor Green commented in a late issue of Pittonia. He found that the corolla exhibits a torsion of 90° to the left, and thereby differs materially from that of Cassia proper. Other characters are adduced to prove the distinctness of the two genera.

SPECIES CHARACTERS AMONG THE VIOLETS. By CHARLES LOUIS POLLARD, Washington, D. C.

NOTES ON ARCTIC WILLOWS. By W. W. ROWLEE, Ithaca, N. Y.

THE arctic willows of North America and Greenland present several difficult problems. In most collections the material is meagre and fragmentary. Often one is in doubt as to whether a staminate and pistillate belong to the same or different species. Even Andersson with the best collection in the world in his hands made mistakes of this kind. Few who are particularly interested in willows have had the opportunity to study the plants in the field. The Cornell party on the Peary expedition of 1896 brought back an exceptionally good collection of willows and the specimens discussed in this paper are from this collection. The glaucoid and myrtilloid groups, perhaps the most difficult to segregate of all, have several interesting forms. The paper attempts to characterize the forms of *S. glauca* L. and *S. grœnlandica*.

One peculiar feature of the specimens of *S. grœnlandica* is the apparent reversal of forms that would be expected in passing from south to north on the coast.

SOME STEPS IN THE LIFE-HISTORY OF ASTERS. By EDWARD S. BURGESS, New York, N. Y.

THIS paper presents some results of field-studies of aster variations made during the last twelve years. Its purpose is to review certain known terms in the ontogeny of asters which are liable to misinterpretation.

These sources of confusion, which will be considered, are of three classes, first of which is the number of leaf-forms normally developed at once upon a single stem, including eight principal forms:

(*a*) Primordial leaf, usually roundish and transient.

(*b*) Radical leaves, two or three or more, often progressively different.

(*c*) Lower cauline leaves, usually the most characteristic.

(*d*) Middle cauline leaves, usually transitional in shape.

(*e*) Upper cauline leaves, usually much smaller.

(*f*) Axile leaves, subtending the primary axils.

(*g*) Rameal leaves, on the primary branches.

(*h*) Bractlets, on the ultimate branches.

According as one or the other of the parts of this leaf-series is more strongly developed or is suppressed, the plant will change aspect and may be mistaken for a new species.

The other two sources of confusion, now to be considered, are the normal and the accidental or less usual successive terms in the life-history of the species, here treated both together, distinguished by number and letter, the normal or usual by the letter N; the accidental or less usual by the letter A.

N^1 Seedling stage, usually with two small radical leaves.

N^2 Radical tuft stage, often conspicuous, often remaining some years before developing into N^3.

A^1 Oval-topped stage, frequent in Biotian asters, the normal, cordate radical tuft becoming topped out with smaller, thinner, oval or other non-cordate leaves.

A^2 Plantain-leaf stage, an occasional extreme development from the last, the non-cordate leaves becoming the predominant ones and often resembling *Plantago major* in size and shape.

N^3 Cauline stage, normally following N^2, the radical tuft sending up an erect leafy stem which bears six of the eight leaf-forms before mentioned. But instead of taking this normal course of development, the plant may enter upon any one of the following several stages which I enumerate as accidental or less usual:

A^3 Intercalary stage, when one or more little leaves are intercalated into the series with much larger leaves above and below.

A^4 Arrested stage, when the gradually diminishing normal series of cauline leaves meets sudden arrest from which it never recovers, a succession of little leaves now continuing into the inflorescence.

A^5 Sprout-form, usually with leaves somewhat different in form and size from the type.

A^6 Ramifolial or branch-leaf form, when after suppression of the main stem, one or more branches rise to replace it, with new direction, and the leaves larger and more numerous, but the leaf-form assumed remaining true to the branch-leaf type for that particular species.

A^7 Bifurcation, either in leaf or stem, arising apparently not from accident but as a sport.

A^8 Opposite-leaf state, due to suppression of internodes, especially upon abnormal branches.

A^9 Verticil form, three nodes brought together, in inflorescence or rarely in the leafy stem.

N^4 Æstivation, the budding stage; often a very different aspect is taken here from that before or after.

N^5 Flowering stage, beginning with erect terete rays which are soon tubular by involution, and in anthesis may change greatly according as the following progressions become developed or not:

(*a*) Pedicels lengthen, changing sessile buds into long-pedicelled flower-heads, and dense bunches into loose clusters.

(*b*) Cymose development may prolong the outer branches to overtop the central and original inflorescence.

(*c*) Rays flatten across, becoming flat and rounded.

(*d*) Rays change position from the erect to the horizontal and finally decurved, incurled or pendulous.

(*e*) Rays change color with age, to white, greenish or brownish.

(*f*) Disks change color early, from yellow to red, reddish-brown or brownish.

(*g*) Disks change form from flat to dome-shaped.

(*h*) Pappus changes color by yellowing, darkening or reddening.

A^{10} Enfeebled state, after close cutting down; when the new stems rising from the same rootstocks the next year are often shorter, weaker, scantier and paler in inflorescence and less varied in leaf.

N^6 Resting stage, when instead of the preceding or after it, the rootstock develops radical leaves only for a series of years.

N^7 Surculus stage, a lateral offshoot arising from the preceding rootstock rising and making ready to enter upon the radical-tuft stage N^3 and renew the round of the life-history.

Specific distinctions are hardly to be found in the constant absence or presence of any less usual stages, but rather in the relation the plant bears to them when they are found, the ease with which they are induced or thrown off, and the shapes assumed when induced. Some species habitually elide one or more terms in the series, some acccelerate them, some prolong or accentuate them. An exceptional development does not invalidate a specific character, as its possibility is latent in all.

THE PLEISTOCENE AND PLANT DISTRIBUTION IN IOWA, WITH MAP. By THOMAS H. MACBRIDE, Iowa City, Ia.

AN attempt to show that many peculiarities in plant-distribution in Iowa are explainable by reference to the recent geological history of the state.

ANOTHER SELF-REGISTERING TRANSPIRATION APPARATUS. By EDWIN BINGHAM COPELAND, Monroe, Wis.

DESCRIPTION of a very simple and easily-operated apparatus, consisting of a wheel over which runs a string carrying the plant tested on one end, an aerometer on the other; as the plant loses weight the counterweight, the areometer, sinks. The record is kept as with the auxanometer. One day's record presented to illustrate working of machine.

[Paper will be published in the Botanical Gazette.]

METHODS OF STUDYING THE SAP PRESSURE OF THE SUGAR MAPLE. By L. R. JONES, Burlington, Vt.

THIS paper described the substitution of a self-registering steam pressure gauge for the ordinary mercurial gauge used to record sap pressure, and gave results of some experiments made therewith.

NOTES ON THE PHYSIOLOGY OF THE SPOROPHYTE OF FUNARIA AND OF MNIUM. By Prof. RODNEY H. TRUE, Madison, Wis.

THE growth rate of the sporophyte of these mosses may be represented by a rather flat curve, rising somewhat more gradually than it falls. Subsequent to the breaking loose of the calyptra from the gametophyte, growth is confined to the distal end of the sporophyte and the growing region, about 2.0 mm. long, is entirely included by the calyptra.

The calyptra, much developed in Funaria, less so in Mnium, is a protective structure, chiefly useful in preventing desiccation. In Funaria the cells of the calyptra are living and contain chlorophyll grains. They are probably self-supporting until the calyptra is ruptured and falls off.

The curvature of the seta in these species results as a response to the stimulus of gravitation. In the earlier stages of its growth the seta is not sensitive to this stimulus, but becomes so as the time for the development of the capsule approaches and by use of the mechanism of growth executes the curvature.

The direction of the strongest illumination determines the radius in which the capsule shall fall. In Mnium the capsule falls with great regularity away from the direction of the strongest light, exposing the end bearing the stomata to the light. Occasionally some fall towards the light.

Funaria obeys with much less precision and regularity the same rule. The conduct of these mosses stands in ecological relation to the situations which they characteristically occupy.

THE SEEDS AND SEEDLINGS OF SOME AMENTIFERÆ. By W. W. ROWLEE and GEO. T. HASTINGS, Ithaca, N. Y.

STUDIES of the writers lead to the following conclusions: In Juglans and Hicoria the cotyledons correspond with the valves of the nut and are deeply two lobed. The two divisions of the embryo resembling cotyledons are each made up of valves of the cotyledons. (2) The seeds of Hicoria germinate without frost action; those of Juglans only with frost action. (3) The tap-root is very thick in young seedlings, and very long in older ones. (4) In Castanea and Quercus the shell is split by a swelling of the cotyledons in germination. (5) In the species of Quercus studied, the leaves of the seedlings were much alike, and not deeply cut or lobed. (6) Fagus is the only one in which the hypocotyl lengthens, or the cotyledons become aërial.

THE MORPHOLOGY AND TAXONOMIC VALUE OF THE FRUITS OF GRASSES. By P. BEVERIDGE KENNEDY, Ithaca, N. Y.

THE embryo of the fruits of the Gramineæ stands isolated from those of other monocotyledons by possessing the peculiar structures called the epiblast and plumule sheath.

It was with a view to determine the constancy, morphological significance and taxonomic value of these parts that led to the following results from the investigation of about eighty genera.

The tendency appears to be that the tribes Maydeæ, Andropogoneæ, Zoysieæ, Tristegineæ are without epiblasts; while the Oryzeæ, Agrostideæ, Aveneæ, Chlorideæ, Phalarideæ, Festuceæ, and perhaps the Bambuseæ possess epiblasts. Peculiar exceptions to this occur, however, in some tribes. The tribe Hordeæ appears to have equally as many with as without epiblasts.

The writer is led to believe from the study of the perfectly developed epiblasts found in Zizania, Leersia and Oryza that the epiblast is a second rudimentary cotyledon opposite to the scutellum (cotyledon).

The plumule sheath is constant in all embryos and from a study of the vascular system, together with Hanstein's investigations on the development of the embryo of Brachypodium, the writer believes that it is a ligule-like growth belonging to the scutellum (cotyledon) and is homologous with the ligule of the fully developed grass leaf.

Unlike Bentham and Hackel the writer is inclined to believe that the Bambuseæ and Oryzeæ together represent the most primitive grasses.

The Oryzeæ resemble the Bambuseæ as follows:

1. They show great variation in the structure of their fruit and spikelet.
2. They possess remarkably large epiblasts.
3. Some have the same number of lodicules.

4. Pharus has a style with three stigmas

5. Many of the genera have broad petiolate leaves and transitions between these to linear ones.

6. They have to a great extent the same geographical distribution, the larger number of the genera being indigenous to tropical America.

The tribes Zoysieæ, Tristegineæ, Andropogoneæ, Maydeæ and Paniceæ both according to the characters of the fruit and those of the inflorescence according to Hackel's classification, form another natural group joined to the Oryzeæ through Zoysieæ and Tristegineæ.

The Chlorideæ, although regarded both by Hackel and Warming as being removed some distance from the Andropogoneæ have been found like them in their fruit characters. The remaining tribes Phalarideæ, Agrostideæ, Aveneæ, Festuceæ and Hordeæ from their fruit characters form another natural group in the order named which coincides with the classification given by Hackel.

THE CARYOPSIS OF THE GRAMINEÆ. By Prof. L. H. PAMMEL, State Agricultural College, Ames, Iowa.

THE ECOLOGICAL DISTRIBUTION OF COLORADO AND WYOMING PLANTS. By Prof. L. H. PAMMEL, Ames, Iowa.

FERTILIZATION OF THE MUSKMELON FLOWER. By Prof. F. WILLIAM RANE, Durham, N. H.

NOTES ON DESTROYING COMPTONIA ASPLENIFOLIA. By Prof. F. WILLIAM RANE.

LENGTH OF TIME FROM BLOSSOMING UNTIL SEED DEVELOPMENT OF LEUCANTHEMUM VULGARE. By Prof. F. WILLIAM RANE.

THE WORK PERFORMED BY THE AGRICULTURAL COLLEGE TOWARD A BOTANICAL SURVEY OF MICHIGAN. By W. J. BEAL, Agricultural College, Mich.

STUDIES RELATIVE TO THE PERIGYNIUM OF THE GENUS CAREX. By Dr. K. M. WIEGAND, Cornell Univ., Ithaca, N. Y.

OBSERVATIONS ON SOME HYBRIDS BETWEEN DROSERA INTERMEDIA AND DROSERA FILIFORMIS. By Prof. J. M. MACFARLANE, University of Pennsylvania.

SECTION H.

ANTHROPOLOGY.

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ADDRESS

BY

J. McKEEN CATTELL,

VICE PRESIDENT, AND CHAIRMAN OF SECTION H.

THE ADVANCE OF PSYCHOLOGY.

THE American Association for the Advancement of Science, when it first met fifty years ago, had no place for anthropology nor for psychology. Science has its origin in the practical needs of society. In a new country of great area and rich resources the energies of its people were directed to invention and exploration. The spirit of Franklin led to the development of railways, steamboats and telegraphy, to the building of cities and the search for mines.

But not only in America and in the case of the anthropological sciences have fifty years brought great changes. Science has become a leading factor in modern life by the rapidity rather than by the duration of its growth. Our own revered Dr. Hall might have talked with Herschel, who could almost have touched the hand of Newton. Newton was born the year that Galileo died. If Copernicus had lived to extreme old age he might have handed the *De Orbium Celestium Revolutionibus* to Galileo. While the whole history of science is thus nearly spanned by five lives, modern science is scarcely older than our Association. It was only in the decade of its foundation that the doctrine of the conservation of energy was announced, while the *Origin of Species* was published in the year of our eleventh meeting. If the physical sciences, as we now understand them, are scarcely more than fifty years old, and the biological sciences are only forty years old, the anthropological sciences are still younger. Perhaps these are now in the condition

of the physical sciences before they had become truly exact, of the biological sciences before they had become truly genetic.

It is easy to prophesy after the event, and from our present point of view science in its history appears to have followed a necessary course. The phenomena of the physical world are stable and readily subject to experiment and measurement; their control is essential to material progress. It is, therefore, no wonder that the physical sciences should have preceded the biological sciences in their development. Far more complex, transient and inaccessible to experiment even than the phenomena of living beings are men, they themselves and their deeds — sciences of these things must come late. As man has been evolved from simpler forms of life, and these were preceded by a lifeless earth, so the sciences dealing with man, with life and with matter must be based one upon the other. The history of science is, therefore, full of promise for the student of anthropological science. We may grant the past to others and claim the twentieth century for our heritage. Perhaps our Copernicus, our Newton, our Lavoisier, our Darwin is one of the younger members of this Section.

It may, however, be that the subject-matter of the anthropological sciences is such that they will proceed without catastrophe. The form of the earth is altered by earthquakes on occasion, but every day it is molded to a greater degree by less obtrusive agencies. In the making of the anthropological sciences by the collecting and arranging of facts, by discovery and by generalization we have every reason to be satisfied with the part taken by America and by this Association. In many sciences we must at once yield the leadership to older nations. In the youngest of the sciences we at least stand on terms of equal service. It was perhaps by special providence that last year's meeting of our Association was presided over by an anthropologist, but this year we chose to celebrate our fiftieth anniversary under a member of this Section. That this Section should have supplied three of our last five presiding officers and our permanent secretary for twenty-five years; that the British Association should have come to America to establish its Section of Anthropology and on its second visit should have chosen an anthropologist for its president — these things we may at least regard as an omen of the place of anthropology in the science that is to be and of the place of America in Anthropology.

While anthropology, largely owing to the richness of the material at hand and to the great Bureau of American Ethnology established to investigate this material, has come to such fruition in America, psychology has also shown great vitality. Unlike anthropology, psychology is distinctly a university discipline. For reasons chiefly theological, ethical and educational, mental and moral philosophy had been taught from the foundation of our colleges by the president of each institution to all students. It happened that at the same time that psychology had developed into a science, the college president became an executive officer, no longer chosen as a matter of course from the clergy, having neither time nor training for the teaching of mental science. There was thus an empty place which the modern psychologist was prepared to fill. Further, the rapid development of the college into the university with elective courses permitted psychology to compete on equal terms with the older sciences, and the result has been its secure establishment in the university. Indeed, psychology touching with one hand philosophy, the humanities and the historical and political sciences, while with the other hand it reaches toward the natural and exact sciences, bids fair to become central. Thus, at Harvard, Professor Münsterberg's elective course in empirical psychology was this year followed by three hundred and sixty-five students. At Yale Dr. Scripture's course in physiological and experimental psychology was elected by one hundred and thirty-eight students. There were last year given by American universities eighteen doctorates with psychology as the major subject — more than in any science except chemistry; six times as many as in astronomy, and nine times as many as in anthropology.

Psychology, though its recent development has been so rapid, is not a new science; it should rather be regarded as one of the oldest of the sciences. Under the great dynasty of philosophers— founded in Greece, not extinct even during the Dark Ages, reestablished after the Renaissance — all the sciences were developed. From Aristotle to Kant the history of philosophy is in large measure the history of science. But as the domain of knowledge became too great ruled by a single mind it must needs be divided into the principalities that we call the sciences. From the beginning psychology has been the favored nursling of philosophy and, as the other sciences were taken from it, all the more did it cherish that which was left. In Great Britain, Locke, Berkeley

and Hume and their English and Scottish successors to the present time have been at once students of philosophy and of psychology. The same may be said of Herbart, Lotze, Wundt and many more in Germany and in France. In our own country to-day we find many of our leaders—James, Ladd, Royce, Dewey, Fullerton and others—professing equally philosophy and psychology.

Psychology, under the guidance of philosophy, became at times somewhat schematic and unreal, though never I think unfruitful or regressive. It needed, however, to be cross-fertilized with the natural sciences. These sciences, in their development, could not ignore the senses and the mind. Perceptions are parts of a physical system, but they are also parts of an individual consciousness. Newton not only analyzed light, but also named seven colors; Dalton found that he was blind to certain of them; Young invented a theory to account for their combinations; von Helmholtz reviewed the phenomena in one of the greatest of books. Physiology on its side could not study the senses and brain while ignoring the functions that they subserve, and it was found that the methods of natural science could be applied in psychology. The zoologist could not neglect the mental life of animals and the place of consciousness in evolution. Darwin wrote not only *The Origin of Species*, but also the *Descent of Man* and *The Expression of the Emotions*, and was the first to study the development of the child's mind.

The subject-matter having been supplied by philosophy and the methods by natural science, the way was made ready for the establishment of a science of psychology. This we owe largely to the intellectual patience of Germany, tired at last of the Hegelian metaphysics. Lotze published his Medizinische Psychologie in 1852; Fechner his *Elemente der Psychophysik* in 1860; Wundt his *Menschen und Thierseele* in 1863, and his *Physiologische Psychologie* in 1874. Lotze and Wundt were doctors of medicine; Fechner was a professor of physics; they were all deeply interested in philosophy. Psychology, as pursued to-day, is deeply indebted to these three, perhaps chiefly to Wundt, who, continuing his services, founded in 1879 the first laboratory of psychology, and established in 1883 a journal for the publication of its investigations. Following and leading this psychological movement we find in Germany men such as Stumpf, Lipps, Ziehen, Müller, Ebbinghaus and many more. One of them — Münsterberg — has

come to us, while Höffding, of Denmark, may be included in the same group. Physiologists, such as Hering, Aubert, Preyer, Flechsig, Exner and von Kries, and physicists, such as von Helmholtz, Mach and König, may reasonably be claimed, in part at least, for psychology.

In France a philosopher, as Cousin, or a man of letters, as Taine, may have accomplished somewhat, but modern writers and workers, as Ribot and Binet, have been largely influenced by Germany. France has, however, made two independent contributions of importance, though in both cases chaff has been mixed with the grain. These are, on the one hand, abnormal and pathological psychology and, on the other hand, individual and social psychology. In Italy the names of Lombroso and Mosso remind us of work in criminology and in physiology that has become important for psychology, as is also the case with the histological work of the Spaniard, Rámon y Cajal. In Russia performance waits on promise.

Great Britain has developed a modern psychology without breaking with its traditions. It has always been a land of great individuals, and Locke, Berkeley and Hume have found worthy successors in Bain, Ward and Stout, to mention only three living psychologists belonging to three different generations. Darwin, Huxley and Spencer, while establishing the theory of evolution, gave mind its due place, Spencer having concerned himself especially with mental and social evolution. Romanes and Lloyd Morgan have directed attention to the study of animal intelligence, while Galton's contributions to anthropometry and heredity have exhibited great originality. Experimental psychology has not made much headway in Great Britain. They have let us take from them Titchener; so he must count for America. The first laboratory, at Cambridge, was founded by me and maintains a humble existence. Within a year a laboratory has been founded at University College, London, and Oxford has at least awakened to the existence of experimental psychology, to the extent of decreeing this summer that it shall not be taught under a newly established lectureship of mental philosophy.

This American Association is naturally most concerned with what has been accomplished for psychology in America. The history of psychology here prior to 1880 could be set forth as briefly as the alleged chapter on snakes, in a natural history of Iceland—"There

are no snakes in Iceland." The eminence of the theologian Jonathan Edwards is witness of the lack of any psychologist. We had only text-books by college presidents setting forth Scottish realism. Porter, of Yale, gave us the best of these books, but McCosh, of Princeton performed a greater service by placing the ægis of theological conservatism over both organic evolution and experimental psychology.

But the land lay fallow and twenty years ago the seed was sown. James, at Harvard, began the publication of a series of striking articles, culminating in the issue, in 1890, of *The Principles of Psychology*, a work of genius such as is rare in any science or in any country. Hall, in 1883, established in the Johns Hopkins University, the first American laboratory of psychology—the second in the world[1]—since transferred to Clark University. *The American Journal of Psychology*, founded by him in 1887, shows the value of the work accomplished at Johns Hopkins and Clark, while the Pedagogical Seminar bears witness to his influence in stimulating interest in the child's development. Ladd published, in 1887, his *Elements of Physiological Psychology*, a work that has exerted great influence here and abroad. It has been followed by a series of treatises on psychology that have never been equalled in range and thoroughness.

Under James, Hall and Ladd and in Germany there was then trained a second generation of psychologists, and we ourselves now have our students, a third generation. The second American laboratory was under my direction at the University of Pennsylvania. The first chair in psychology was there established and courses in laboratory work were there first given. This was only ten years ago. The work at Pennsylvania is continued under Witmer, while at Columbia we have since 1890 a laboratory and a range of courses in experimental, physiological and analytic psychology which may bear comparison with those of the other sciences. Baldwin carried modern psychology, himself enlarging it as he went, from Princeton to Lake Forest and thence to Toronto, and back to Princeton. The laboratory established by him at Toronto is continued under Kirschmann, while at Princeton he has founded an efficient laboratory, and by his work on genetic psychology has brought our science into fruit-

[1] It would be more correct to regard the laboratory established about the year 1875 by James in the Scientific School of Harvard University as the world's first psychological laboratory. It was, however, nominally under physiological auspices.

ful relations with zoölogy and sociology. Jastrow, at Wisconsin, stands forward as a leader in experimental psychology, having accomplished much for the advancement of the science and the diffusion of its results. In the latter connection may be mentioned the psychological laboratory under his direction at the Columbian Exposition and the fact that almost alone he has represented psychology in this Association.

The great services for psychology of James at Harvard, Hall at Clark and Ladd at Yale were not confined to their own publications, but were also directed towards building up strong departments. Harvard, with James, Münsterberg, Royce, Santayana, MacDougal and other men in closely related departments, surpasses every other university in the world in its opportunity for psychological study and research, and there is quite as much reason for German students to come to Harvard as for our students to go to Germany. Clark University, where Sanford has charge of the laboratory, may almost be said to be a university for the advancement of psychology. The laboratory at Yale, under Scripture, has shown great activity and is able to publish annually a volume of researches. Cornell, under a president who is a student of philosophy and psychology, robbed Great Britain of its best experimental psychologist, and under Titchener the laboratory stands quite to the front, while the courses in philosophy and psychology are especially well developed. Chicago early recognized the importance of psychology. Securing Dewey and Mead from Michigan and Angell for experimental work from Minnesota, in psychology, as in other departments, its development has been marvellous. Michigan and Minnesota have found other men to continue the work, while Stanford, California, Indiana, Iowa, Illinois and all the great State universities have established laboratories and given an adequate place to psychological courses.

It would be possible for me to give other objective proofs of the progress of psychology—the establishment, in 1894, of a second journal, *The Psychological Review*, which during the first six months of the present year has published some 900 pages, all we hope contributing to the advancement of psychology; the prominent place given to psychology in our journals of education, philosophy, general science and popular literature; the widespread amateur interest in child-study and psychical research; our Psychological Association, with a membership of 102, all engaged in advancing

psychology — these and other witnesses for psychology might be called upon to testify, but the development of psychology in the university has seemed to me best deserving of extended comment. Until the State shall learn to care for those who do the most for it, until those who are engaged in advancing knowledge shall work for the State and be supported by the State, our universities will be the centers for research, and the position of a science in the university will measure its opportunity and fruitfulness. Very significant for psychology, therefore, is its progress in the universities of America during the past twenty years.

My remarks have been confined to the externals of psychology. Its inner history, its present content, its future outlook are not subjects that can be readily brought up and dismissed in a few words. Psychology is the most complex of the sciences. I do not at all claim that it is the most important of the sciences. The human race got on without it very well and could doubtless continue to do so. Its practical applications do not compare in importance with those of many of the sciences; it is in a way lacking in great discoveries and universal laws. But, compared with psychology, a science such as astronomy may almost be regarded as naïve. The entire known performance of the solar system and of the fixed stars since the time of the Chaldeans is less complicated than the play of a child in its nursery for a single day. The stars are so far away, the telescopes are so big, eclipse expeditions proceed to such remote quarters of the earth, that the simplest items of information take on a dramatic interest. Atoms and molecules are so invisible, the ether is so intangible, we know after all so little about them, that it is easy to invent hypotheses that do not contradict our ignorance. The generalizations of physical science are, indeed, the greatest achievements of the human intellect; but the intellect, by which and for which these generalizations have been created, when itself made the subject-matter of a science, is complex beyond those sciences which are its offspring.

Psychology does not, of course, claim as its subject-matter all "that is in heaven above, or that is in the earth beneath, or that is in the waters beneath the earth." The physical and natural sciences, language, literature and the fine arts, industries and institutions have, it is true, their origin and end in the mind, but this does not turn them into departments of psychology. The physicist no more needs to concern himself with the mental processes

leading to his discovery than does the hen with the processes preceding the laying of its egg. The kind of novel called psychological is by no means a product of science. The enjoyment of art decreases as we analyze its products. Scientific prevision and guidance have scarcely more to do with the rise and decline of institutions than with the rise and decline of the sun in the firmament.

Still I do claim that there is no department of knowledge or activity which does not have an aspect that concerns psychology, and while hitherto it is psychology which has learned from sciences preceding it in their development, the time will come, and perhaps has now come, when every science must take into account the facts and theories of psychology. The increase of knowledge which has caused the creation of the separate sciences is now leading to further specialization, but not in the sense of one science more completely ignoring the others. Rather we have at present well established sciences, such as astrophysics, physical chemistry, biology and others, which take their subject-matter from two or more of the older sciences. It is probable that students in increasing numbers will take up the inter-relations of psychology with other sciences. Indeed, this tendency is already well marked. Mathematics is necessary as a tool for psychology, while conversely several books have recently been published concerned with the psychological presuppositions of mathematics. The fundamental concepts of mechanics have been treated as mental products by Mach, Pearson and others. At the present moment the Vice-President is giving before the Section of Physics an address on the perception of light and color, while the same subject was chosen by Sir George Stokes last month for the subject of his presidential address before the Victoria Institute. Physics is, however, leaving to psychology vision, hearing, etc., though in the meantime new departments, destined perhaps to become new sciences—represented by the great works of von Helmholtz, *Physiologische Optik* and *Tonempfindungen*—are being created, which draw their materials in equal measure from physics, from physiology and from psychology. Errors of observation, the personal equation, the relation between mental and physical intensity, are subjects where the investigations of the psychological laboratory must be applied in astronomy and the other physical sciences. Chemistry, geology and botany perhaps stand most remote from

psychology. Still, if physiology is in large measure the chemistry of living tissues, chemistry may be brought into intimate relations with psychology. Geology and anthropology are closely related, while physical geography is especially concerned with the relations of man to the earth. Many of the problems of evolution, so essential for psychology, are best studied in plants, and the applications of botany in agriculture, etc., are determining factors in the evolution of man.

Physiological psychology is already accepted as a distinct discipline, while zoölogy and psychology are equally intertwined. It is impossible to separate physical and mental evolution — witness the writings of Darwin, Spencer, Huxley, Romanes, Cope, Morgan, Baldwin and many more. Under the classification of this Association, anthropology and psychology are included in one section. Several of our leading members would make psychology a branch of anthropology, while psychology regards ethnology and archæology, on the one hand, and somatology, on the other, as contributing much to its subject-matter. Sociologists find it somewhat difficult to disentangle their field from that of psychology.

If we turn to the departments of knowledge not represented in this Association — the humanities — we find psychology to be a connecting link between them and the physical and natural sciences. If we regard the professions — medicine, law, theology, teaching, journalism — we find that their products when systematized into sciences give to psychology and take from it. Literature and the fine arts, both in their origin and in their end, may be studied as departments of psychology, though it does not appear that psychology has as yet been of great service to them.

I may illustrate the interdependence of psychology and other sciences by a definite example. Much is being written just now regarding the relation of consciousness to the brain. The question is: Do perceptions, thoughts, feelings, volitions, stand in causal interaction with the brain, or are they an epiphenomenon, accompanying changes in the brain but not influencing them? Are our ordinary actions complex reflexes due to physical stimuli and the structure of the nervous system, or are the changes in the brain that precede movements initiated and directed by consciousness? The question is one of facts that should be settled by scientific methods; and the solution will by no means concern psychology alone. The two greatest scientific generalizations of the

present century are the conservation of energy and evolution by survival of the fit. Now, if consciousness alters, however slightly, the position of molecules in the brain the fundamental concept of physical science must be abandoned. If consciousness have no concern in the actions of the individual we have one of the most complex results of evolution developed apart from the survival of useful variations, and the Darwinian theory has failed. Surely both the physicist and the biologist must watch the steps toward the solution of a problem that concerns them so nearly.

The world is one world; every part of it is in relation to every other part, and each part consists in these relations. As a hand cut off from the rest of the body is no longer a hand, as a man apart from other men is no longer a man, so each science and each scientific fact and law has its value and even its existenee in its relation to the totality of knowledge and of life. Psychology has become an integral part of modern science; it gives and takes with a free hand. A parvenu among the sciences, it is self-conscious and knows its obligations and its limitations; but its position in the body scientific is henceforth secure.

I have said that psychology is in a way lacking in great discoveries and universal laws. If I were addressing an audience of psychologists it might be desirable to consider whether this is due to the subject-matter or only to the immaturity of our science. Under present circumstances it is perhaps better in part to question my own statement. Columbus discovered a new world; Copernicus discovered innumerable worlds; but Descartes discovered, or at all events invented, the soul. Which after all was the greatest scientific advance? Columbus did not foresee four hundred years of history, the present unequal conflict of a powerful nation with a declining civilization. Descartes' ideas of the relation of mind and body are not ours. But is not the very subject-matter of psychology one of the greatest discoveries of modern science? To unite strict idealism with strict materialism; to give consciousness its central place in the universe and yet to show that each change in consciousness is correlated with a change in the nervous system— this I claim to be a scientific generalization comparable to that of the conservation of energy or of organic evolution.

Minor but clear-cut discoveries in psychology have not been lacking — witness color blindness, individual types of mental imagery, the dependence of emotion on reflex bodily movements, hyp-

notism, etc. Neither are quantitative formulas denoting relations among mental states or between mental and physical change lacking. Recent researches on subjects such as the perception of space, illusions of sense, color-vision, the time of mental processes, memory, fatigue and many more represent scientific advances as definite, interesting and important as those of physics or of zoölogy. Psychology has been able to adopt the quantitative methods of exact science and the genetic methods of natural science, while its older methods of description and analysis witness an insight and acuteness unrivalled by any other science.

I have said that the practical applications of psychology do not compare in importance with those of many of the other sciences. Here again, while a society of psychologists might properly discuss the causes of this limitation, in an address confined to generalities, it may be more profitable to point out that daily life consists in the application of such psychological knowledge as is at hand. How could Bismarck and Gladstone direct contemporary history except by superior insight into the way men act and the methods of influencing their actions? What have Wagner and Browning done except excite interest and emotion? The conduct of every profession and of every business is chiefly based on the adjustment of thoughts, feelings and actions. Systems of government and education are simply methods for controlling and directing the human mind. Now, of course, all this is done by the rule of thumb entirely uninfluenced by psychology as a science. The savage who kills a bird with a stone is not thereby shown to be a zoölogist and a physicist. Still he does have a kind of knowledge of the habits of animals and of the laws of projectiles, whence have developed the sciences, which in the course of time have turned back to daily life those applications of science in which modern civilization consists. Whether the history of the material sciences will be repeated in the case of the mental and social sciences it is not possible to say or to gainsay. There are at present indications of the application of psychology in the treatment of diseases, in education and in other directions. Evolution, careless of the individual, has proceeded with boundless waste; certainly we are now interfering with its course for our benefit. It may be that some day the applications of material science will be subordinate to those of psychology.

These things lie on the knees of the gods. What the future will

bring we do not know, but the past is ours. When we regard the fifty years of this Association or the century now ending, we cannot fail to see that it has been an era of science. German music, English poetry, the modern novel — these are great achievements, but scarcely comparable to the forward movement in science. The older sciences have been reformed and new departments have been established. But amid all this scientific progress nothing has been more notable — at least from my own partial point of view — than the development of psychology into a science rivalling in activity and fruitfulness the other great sciences.

PAPERS READ.

[ABSTRACTS.]

TYPOLOGICAL ANALYSIS. By DANIEL G. BRINTON, M.D , Media, Pa.

THE profitable study of museum specimens rests upon the correct method of analyzing the types presented. Some have claimed that they should be studied as species and genera, after the manner of botanical or zoölogical examples. Others prefer to arrange them in "developmental" series, illustrating by them the supposed laws of evolution. While not fundamentally erroneous, these natural-historical methods tend to obscure the chief purpose of such specimens, which is the illustration of human, collective psychic processes and ideas. "Developmental" series are rarely accurate, as they are made up usually from several culture centres. Psychical is not identical with morphological development. Examples may be drawn from the products of the useful and the fine arts.

PAPAGO MEDICINE. By W J MCGEE, Museum of American Ethnology, Washington, D. C.

THE SMITH SOUND ESKIMOS; THEIR RELATIONS TO OTHER ESKIMO TRIBES. By A. L. KROEBER, Columbia University, New York, N. Y.

THE location, and consequent apparently absolute isolation, of the Smith Sound Eskimo, render their relationships of interest. The possible affinities are: (1) With the Central Eskimo of Baffin Land, etc.; (2) with the Greenlanders; (3) with the separate tribe of East Greenlanders.

Language. The language of the Smith Sound tribes is almost pure Greenlandish, with two modifications.

1. Phonetic, agreeing with the dialect of the Central Eskimo.
2. In the verb-inflection, agreeing with Mackenzie River and Alaskan dialects.

Shaman Language. Seems to resemble that of Greenland most nearly.

Tales and Mythology. Intermediate between those of Greenland and the central regions. There is on the whole, perhaps, a closer resemblance between Greenland and Smith Sound versions of the same tale than be-

tween the Central and Smith Sound versions, but the latter two have several tales or beliefs in common not found in Greenland.

History. Their history is an absolute blank until their discovery by the European. Subsequent intercourse with one family of Central Eskimo, however, has restored to them the kayak and the bow, the use of both of which had become lost to them.

Customs and Social Life. Give little clew as to their affinities.

Summary. Everything seems to show that the Smith Sound Eskimo are intermediary between those of Greenland and the Central Regions. They are not to be regarded as a side-branch of the Central Eskimo, nor have they close affinities with the East Greenlanders. The chain of ethnic relationship is: 1. Central Eskimo; 2. Smith Sound tribe; 3. Greenlanders; 4. East Greenlanders.

BURIAL CUSTOMS OF THE ANCIENT ZAPOTECANS OF SOUTHERN MEXICO. By M. H. SAVILLE, American Museum of Natural History, New York, N. Y.

ART IN PREHISTORIC TIMES. By THOMAS WILSON, U. S. National Museum, Washington, D. C.

MANY volumes have been written on the History of Art, the Origin of Art, the Dawn of Art, the Beginning of Art, the Evolution of Art, and the last one, just appeared, What is Art? Some of these have been large heavy works of two volumes, attempting to cover the world in their investigations, and treating of art in every style and in every nation. All of these, with a single exception, begin with historic art, like Chaldean, Assyrian or Egyptian, practically ignoring prehistoric art. I have sought to remedy this in a new work now going through the press, entitled "Art in Prehistoric Times." Its entire field lies in the prehistoric, and is earlier than any of the works mentioned; indeed, mine ends where they begin.

A pertinent preliminary question is posed by Tolstoi in his work, "What is Art?" But first, let us say what is not. It is not necessarily to be measured by the beauty of its works nor the amount of pleasure they give the beholder or listener. Indeed, it may be the reverse. A work of art may derive its principal value from the amount of awe and fear it produces. Wiertz was an excellent painter, but his gallery at Brussels is little better than a chamber of horrors. Shakespeare's tragedies, rendered by accomplished actors, excite emotions of horror and dread. The master musician sometimes extorts the warmest plaudits by playing a scale or number purposely false, thereby showing his art by his power over his voice or instrument.

Darwin, Spencer, Schiller, and others have defined art, but there is something faulty in nearly all their definitions. Véron says art is the manifes-

tation of human emotions externally interpreted by expressive arrangements of line, form or color, or by a series of gestures, sounds or words, governed by particular rhythmical cadences. To this Tolstoi adds the condition that the second party, the beholder or listener, must be the recipient of these emotions. This definition would revive the ancient discussion as to what is sound, wherein it was agreed that making waves of air was necessary, but suppose, as in the case of a tree falling in the distant wood, there was no one to hear it; certainly it would be sound whether heard or not. So the other would be art, even without a beholder or listener.

Emotions, whether grave or gay, are interpreted or manifested in some of the following methods: when by color, the art is painting; when by line, it is drawing or engraving; when by form it is sculpture; and a combination of these may produce architecture. When the emotion is manifested by gesture or human rhythmic movements, it produces the dance; when by rhythmic notes, music; when by rhythmic words, poetry. Certain of the arts appeal to the brain through the organs of sight: painting, sculpture, engraving, architecture, and the dance; while certain others appeal through the organs of hearing: poetry, music and the drama. In the former group action is arrested and the representation is confined to a single moment of time; in the latter group the action is continuous. But the laws as to art govern both groups alike. A work of art is the ideal of the artist. It is his own thought or idea, is a part of himself; not nature as it actually is, but as he sees it, as he idealizes and depicts it.

The great, if not the principal, difference in the ability of artists or authors lies in the difference in their ideals, which is only another name for their taste. Taste is born with the individual and, to a large extent, is involuntary. True, it can be practised and improved, and by want of thought or use it may be deteriorated. But with all this it has its limits. One who cannot naturally and easily detect concords or discords of sound can never become a good musician; and he who cannot recognize harmonies of color will never become a great painter.

Despite all arguments favoring the evolution of man as a species, we must recognize that ever since man has been known on earth he has had all the powers and capacities of man and, along with them, the taste and ideality needed for an artist. So soon as he came to know art, to have a taste and an ideal, he seems to have had a desire for art objects. A desire to have them appears to have been an equivalent to a desire to produce them, and so man began to make art works in times of high antiquity. Art seems to have been the germ of civilization, rather than to have been its flower or fruit. This is no *a priori* theory, for investigating the facts and interrogating his former habitat with pick and shovel, we find that he actually made and had many pieces of considerable beauty, even if of no value, purely for ornament and consequently to gratify his æsthetic taste and give him pleasure. This puts him on the level with modern man and ascribes to him the same motives and tastes which influence modern man to do his art work.

These manifestations of æsthetic art began in the earliest known period of man's civilization on earth, the Paleolithic. Whatever differences there may be among prehistoric archæologists as to details, they agree in the most important fact, *i. e.*, that this human occupation of the earth was to be traced to the quaternary geologic period. Its antiquity is not to be measured in years nor by any of our time units. It is simply to be described as a prior geologic period. The human race was then in its infancy. The man of that time was a savage. He did not bury his dead, he built no houses, he erected no monuments; he was a hunter and fisher, he was a nomad, had no local habitation, dwelt in no villages except such as could be so called from a number of people living in a cavern for shelter. He had no tribal organization, no sociology, no belief in a future state and, consequently, no religion; at least no evidence of any of these things has ever been found associated with his remains, while they have been found among his successors. Yet this man occupied in the Solutréen epoch, the highest rank as a flint chipper, and in the succeeding, the Madelainien epoch, a correspondingly high rank as an engraver on bone and ivory. His material (in the latter epoch) was the bones, horns and tusks of the animals which were his prey. His tools were sharply chipped points or gravers of flint. Most of the specimens of his art work are found in the caverns which had been his habitations. No one has sufficient knowledge to justify the declaration that this art work belongs exclusively to western Europe, but certain it is that most known specimens have been found there. They are chiefly from the caverns of central and southern France. About four hundred such specimens have been found and preserved, yet no one can know how many have been missed or remained undiscovered. The specimens found in caverns were originally thrown aside, lost in the débris, and finally protected by stalagmitic or other process of induration. They are usually enveloped in blocks or slabs which, by infiltration, have become hard and must be quarried like stone. In bringing these to light in modern investigations many specimens must necessarily be broken, while those in the interior of the block or slab are never discovered. A few specimens of the art work of the Paleolithic period are purely decorative, but most of them are representations of the animals of the period of the locality. Many animals, now extinct, are represented and in this way knowledge of their form and appearance has been preserved to us. The animals most frequently engraved were the mammoth, cave bear, Irish elk, musk ox, reindeer, chamois, mountain goat, urus or auroch, horse, deer, and similar animals, finally man. Marine animals were well represented: the seal, sea lion, tortoise, turtle, fishes and serpents. Some of the objects thus treated were utilitarian; *batons de commandment*, poniard or dagger handles, harpoons, and similar objects; while others were purely ornamental, only toys. Many were so broken as to destroy all clew to their purpose. This was the art of the Paleolithic period.

That of the succeeding, the Neolithic and bronze ages (of Europe), was quite a different style. It was almost entirely decorative, marked or cut

on pottery and bronze objects of utility. There was little or no attempt to represent living or material objects. The art of these periods (still in Europe) consisted mostly in geometric forms, as lozenges, chevrons, herring-bones, zigzags and cross hatch. The Swastika seems to have been used throughout the latter period and to have been the earliest symbol in use among men.

The North American Indian and his congeners of the Antilles were also in the Neolithic or polished stone age of culture, and their decorative art was practically the same as that age in Europe. They excelled their European brethren, however, in making rude drawings and pictures, principally petroglyphs, many of them possibly ideographs, telling a story by their own description. They often reproduced the human figure, which the European rarely did. The aborigines of Mexico, Central, and the northwestern part of South America, although still in the Stone age, reached a higher civilization, mainly manifested by the erection of extensive and magnificent temples, fine stone sculpturing and ideographic language.

In closing, we are tempted to enquire, Why did the savage man take upon himself the additional labor of making, constructing, erecting, all these, to him a savage, unnecessary works and objects? The answer suggested is — because he desired to have them; they gratified him, they gave him pleasure; they satisfied his æsthetic taste for beautiful things; and to obtain the desired gratification, he was willing to undergo the necessary labor and to endure the necessary fatigue. In respect of the desire for art and art works, things beautiful, wherein is the difference between the prehistoric savage who dwelt in caverns, and his modern brother who dwells in a palace?

PREHISTORIC MUSICAL INSTRUMENTS.[1] By THOMAS WILSON, U. S. National Museum, Washington, D. C.[2]

IT has been asserted that the origin of vocal music was coëval with that of language, and that the construction of musical instruments dates with the earliest inventions suggested by human ingenuity. Those who make these assertions do so simply upon theory, and when pressed for their authority would be compelled to admit that actual knowledge or information upon the subject does not exist. What can be affirmed is that sound made by the prehistoric man of the earliest epoch might have been rhythmic and so possessed one of the elements of music. The other elements, melody, dynamics and harmony, followed in the course of civilization, among some people at a faster and among others at a slower rate.

Vocal sounds are incapable in themselves of perpetuation. When the vibrations made by the human voice have ceased, the incident is closed

[1] A résumé of a chapter in "Art in Prehistoric Times" in course of publication.
[2] Assisted by Mr. E. P. Upham.

and the evidence lost. It is, therefore, in the absence of any written testimony, impossible to identify the practice, or even the existence, of vocal music in prehistoric times. We are driven to an exclusive consideration of musical instruments, and if these should fail us, we would be without evidence.

It is the intention, in this paper, to leave aside all speculation as to the origin, theory, practice, philosophy and poetry of music, and to deal only with music as one of the fine arts of prehistoric times; the study thereof to be based upon such knowledge as is obtainable from prehistoric musical instruments.

Musical instruments of percussion are the most simple in the sound or effect produced, and the theory has been accordingly announced that they were the first to have been invented, that is, they were the earliest factors in human culture, and the first to be used among primitive peoples. The next in order of complexity, and accordingly in order of invention and evolution, were wind instruments. Rowbotham (History of Music, Book I) divides them into types, of which his representatives are the drum, the pipe and the lyre. Under the first head fall rattles, gongs, triangles, tam-tams, castanets, tambourines, cymbals, all instruments of percussion; under the second head fall flageolets, flutes, hautboys, clarinets, bassoons, bugles, all wind instruments. Under the third head fall all stringed instruments. He makes these types representatives of three distinct stages of development, through which, in the order named, he says, all prehistoric music has passed. "As in the geologic history of the globe the chalk is never found below the oölite, nor the oölite below the coal, so in the musical history of mankind is the lyre stage never found to precede the pipe stage nor the pipe stage to precede the drum stage. In keeping with this is the fact that the savages sometimes have the drums alone, but never a pipe alone or the lyre alone, for if they have the pipe they have the drum too, and if they have the lyre they always have both the pipe and the drum."

Wallaschek wrote (Primitive Music: Its Origin, and Development, with Songs, Instruments, and Pantomimes of Savage Races) after Rowbotham, and assailed strongly his theory of threefold development of aboriginal musical instruments. He denied the drum to have been the earliest or first invented, and asserted that which is demonstrated in this paper, that wind instruments (whistles, flageolets and horns) antedated drums. He reversed Rowbotham's order of simplicity and argued that the lyre or harp of Africa, formed as it might have been from a bent twig or the two branches of a tree with a string stretched taut between the ends, were not only more primitive and more easily and quickly made; but, as confessed by Rowbotham in his appendix, it was in fact invented and in use among the most degraded savages of the country, that it spread over a large portion of the continent of Africa before the pipe or drum, and that it was in use among many other tribes in connection with pipes, but without drums.

The authors are aware of the mass of literature on the science and practice of music, how historians and discoverers of primitive or savage peo-

ples have reported, in many volumes, the music they have heard and the instruments with which it was made. These have not been followed nor any of their theories adopted, and the sociologic or scientific sides of music among primitive or ancient peoples have been avoided. They have contented themselves with a description of prehistoric instruments and where possible of such notes or tones as could be produced by their manipulation.

Lartet and Christy found prehistoric whistles in the cavern of Laugerie Basse, in the Dordogne District, France. They also found whistles of the same kind in the cavern of Aurignac. It was supposed that both these caverns belonged to the Paleolithic period, and, therefore, they afforded corroborating evidence of the use of these instruments in that period. Later investigations rendered this certain with regard to Laugerie Basse, but made it uncertain with regard to Aurignac. When the first whistle was found, in 1860, in Aurignac, it was reserved by these gentlemen for this confirmation, but afterwards, when, as they say, there had been many discoveries of this kind of instrument, notably those from Laugerie Basse, so that specimens are not now rare in Museums and collections, they felt themselves justified in assuming the existence and use of this instrument in Paleolithic times.

The question will immediately arise whether these were really musical instruments. It has been suggested that they were for calls or signals, and may have been used as the boatswain does his whistle, to direct the movements of men at a distance. This would not be music, and if they were always thus used they would not be musical instruments; but if, on the other hand, they were used to give rhythmical cadence to a dance or song, or for any similar purpose, they would be musical instruments. Any attempt to decide the use to which these objects were put and whether they were or were not musical instruments would be mere speculation. That they might have been used as such, and that hundreds of other similar instruments have been found which were used as musical instruments, justifies this classification and their insertion in this paper.

M. Fetis (Histoire Générale de la Musique) gives an illustration of a pipe from a prehistoric grave of the Neolithic period, near Poitiers, and of which a cast was furnished by M. Lartet. It was rudely constructed from a stag horn, was blown at the end like a flageolet, and had three finger holes equidistant.

The most elaborate, as well as the most beautiful instruments of music belonging to prehistoric times, were the bronze and gold trumpets or horns of Scandinavia. They are not toys, but are of large size, quite as large as any horns of modern times, being sometimes five and even up to eight feet in length, with bell mouths six, eight, and ten inches in diameter. No particular style was adopted, though all specimens agree in the requisites of the horn or trumpet.

A notice of a concert of these instruments in the Copenhagen Museum was published in the Evening Star, February 6, 1896, from the San Francisco Chronicle, which is here inserted:

"An enormous crowd fills the museum and the neighboring square at Copenhagen every midsummer day to listen to a unique concert. A num-

ber of ancient Scandinavian horns, . . . called "luren," are kept in the museum. Of this collection fourteen are in good condition. They have an elegant shape, and the flat metal plates at the mouthpiece show good technical perfection and a developed taste for art. They are in different pieces fitted together. They are of very thin metal and frequently seven feet long. A few years ago it was found out by Dr. Hammerich that they could still be blown or played upon. Their tones resemble those of the tenor horn, and they have a soft but powerful sound. Some are tuned in C and E sharp and others in D, E, or G, and these tunes form an accord, but no "scala." On the balcony in the court of the palace, in which is kept the Northern Museum, two members of the "capella" blow tunes on two of these primeval horns to the delight of the inhabitants."

Professor Starr, of Chicago, says of one of these concerts:

"We had the good fortune to be present. The court of the museum was filled with hearers. Wonderful, is it not, that horns two thousand years old, buried for long centuries in peat bogs, should, after this long silence, still be capable of giving out clear, ringing, even sweet, tones?"

The ancient musical instruments of Ireland, so far brought to the notice of archæologists, are the horn or trumpet, the harp and the bagpipe. The two latter are more modern and are probably not prehistoric. The Museum of Science and Art at Dublin possesses several ancient harps, attractive on account of their historic and national interest, but they are not for us.

M. Paul du Chaillu wrote his interesting work, "The Viking Age," to demonstrate the proposition that the early settlers of Britain and the British Isles were Vikings rather than Anglo-Saxons. He based his theory upon the similarity of the many objects found, respectively, in Britain and Denmark. While his theory has not been accepted generally, yet it must be confessed that the similarity he points out was remarkable. Not the least is it so with regard to the trumpets or horns (luhrs). A fact in this similarity, opposed to Du Chaillu's theory, is that the greater, almost the entire, number of these trumpets are found in Ireland, while they are extremely rare in England.

Diodorus says of the Gauls that they had barbaric trumpets of a special nature which gave a hoarse sound well suited to the din of battle. The use of war trumpets among the Celtic population of western Europe has been more than once mentioned by classic writers, and passages from them have been cited by Sir John Evans, Sir Augustus W. Franks, and others.

The United States National Museum possesses a large, interesting, and valuable collection of musical instruments, which have been obtained primarily through the interest of the late Dr. G. Brown Goode, the Assistant Secretary of the Smithsonian Institution, in charge of the National Museum. Very many of these are extremely primitive and might well have served in prehistoric times. Their use by savages and primitive peoples in the earliest stages of their history shows an almost certain connection with prehistoric times. It is considered proper that they should be noticed among prehistoric instruments.

Prominent among the prehistoric instruments of the United States from which sounds or notes can be obtained, are the bone whistles from ancient graves of the coast of California. East of the Mississippi Valley with a few exceptions there appears to be an almost entire absence of musical instruments. The whistles made from limonite concretions and the stone tubes or trumpets (?) represent all that can be so classed. (See those in the U. S. National Museum and in the Douglas Collection at New York.)

The ancient instruments from Mexico, Central and South America form a most interesting series and show in some degree that music occupied a prominent place in the arts. This is naturally to be expected of a people or peoples with arts so highly developed in other directions, architecture, sculpture, etc. Drums, rattles, horns and trumpets are mentioned by the early Spanish writers in connection with war, religious ceremonies and festivals of various kinds. The whistles or flageolets of pottery form the largest group in our collection and have been deemed worthy of considerable attention. The whistling mechanism in all is identical with that of the modern flageolet and they have a compass of from two to seven or more notes, according to the number of finger holes and their relative sizes. The smaller instruments are mostly caricatures of the human face or figure, or of animals or birds. The larger instruments are like the modern flageolet in form.

The most important group of prehistoric musical instruments, in point of numbers, is from Chiriqui. It embraces drums, rattles and whistles of pottery and a number of bronze bells.

The musical instruments in the U. S. National Museum obtained from Guiana can not properly be classed as prehistoric. It was thought, however, that descriptions of a few primitive instruments belonging to still existing tribes might prove interesting. It appears that only a limited variety of instruments are known to these people. Rattles made of gourds and plaited strips of cane, and whistles or flutes of bone and other material seem to have been all that can be so classed.

The "whistling bottles" from ancient burial places in Peru should not be omitted. These may not have been musical instruments, but they are capable of producing musical sounds or notes.

CONCLUSION.

Certain differences of opinion exist among historians of music as to the scale adopted by primitive peoples in their earliest efforts. One class thinks that it finds in the laws of sound a relationship between notes of different pitch, which harmonize with each other and make a concord of sweet sounds, and demonstrates a scale natural to all men and therefore universal, applying as well to the savage as to the enlightened; that is to say, they believe that when the savage has become sufficiently practised in music to employ a scale of any kind, he will use that belonging to modern and civilized music because that scale is natural to man.

Another class of persons deny this, and express their belief that primitive peoples either did not recognize this concord, or else did not desire it.

These differences of opinion have not been adjusted. When one party shows primitive instruments on which the modern scale can be produced, or shows primitive music written on the modern staff, they are met with the assertion that this is, or may be, the result of straining the instrument to produce the given note.

The authors have taken no side in this controversy, recognizing their rôle to be that of gathering facts rather than indulging in speculation, to the end that their facts may be used by both parties; thus serving in some degree to elucidate these disputed questions.

Great care has been employed in ascertaining the tones or pitch indicated in the staves, that they should represent the normal scale of each instrument. They could be manipulated so as to give a tone higher or lower than normal. Particular pains have been taken to represent upon the staff the notes obtained by using average force in blowing, and to avoid the temptation to raise or lower the pitch in order that the intervals might be more easily represented on the staff or more satisfying to the ear.

The fact that it is rare to find two instruments from a given locality which produce the same tones indicates at least that their construction (musically) was not based on any established or recognized scale. With most of them the intervals and scale appear to have been purely accidental. What results might be obtained if all prehistoric musical instruments could be brought together and studied it is impossible to say.

CLASSIFICATION OF ARROWPOINTS, SPEARHEADS OR KNIVES. By THOMAS WILSON, U. S. Museum, Washington, D. C.

I SAY "or" knives because many of these objects, which for a long time and in many, indeed most, collections, have been classed as arrowpoints or spearheads, may have been used concurrently as knives. Some of them have been exclusively so. This has led me to attempt a classification.

Heretofore there has been little or no method of describing or defining the different kinds of these implements or weapons. The difference between arrowpoints and spearheads has been accorded to individual taste, and without rule or definition, large ones were spears and small ones arrows. Some were with, and some without stems; the same with notches. No practical classification has ever been in use, and whatever comprehensive classification has been attempted is so intricate and with such infinitesimal divisions as to be impracticable and break down with their own weight. Dr. Rau's classification comprised twenty-three different classes and made it difficult to assign the individual specimen to its correct class, and impossible to remember and apply the classification practically to the specimen in hand. This condition induced me to attempt a classification which should be at once comprehensive and practical. I have made only four grand divisions. These are according to forms already well known and segregated. They are: I. Leaf-shaped; II. Triangular; III. Stemmed; and IV. Peculiar forms. The subdivisions are by letters, A, B, C, etc.

Division I, leaf-shaped, has Class A, pointed at both ends; Class B, pointed at one end only, the base being convex, straight or concave; Class C, sharp point, parallel edges, with base straight, concave or convex, chiefly from the Pacific coast.

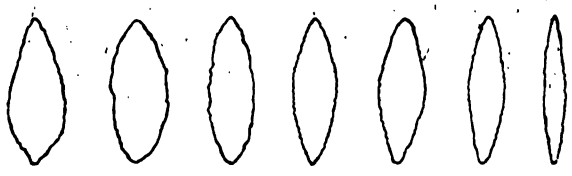
Division II, triangular, has but one class and includes all triangular forms, with edges or bases concave, straight or convex.

Division III, stemmed, includes all stems. Class A, stemmed without shoulders; Class B, stemmed and shouldered without barbs; Class C, stemmed, shouldered and barbed. These comprise all the general or usual forms.

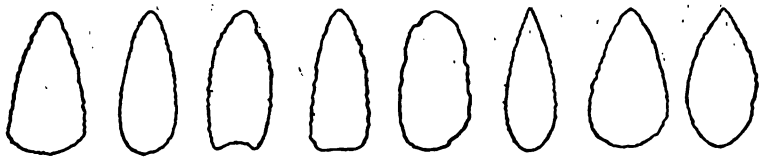
Division IV, peculiar forms, are unusual and many times sporadic, but have differences so marked as not to be included in any of the former classes and are found in such numbers that they cannot be ignored. They commence with A, beveled edges; B, serrated edges, etc., continuing to H, asymmetric or one-sided, and I, curious forms (*forma curiosa*).

DIVISION I. LEAF-SHAPED.

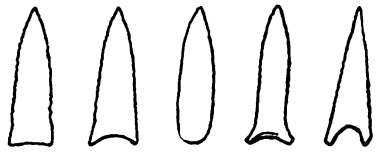
Class A. Pointed at both ends.



Class B. Base convex, straight, or concave.



Class C. Convex, or concave base, parallel edges.

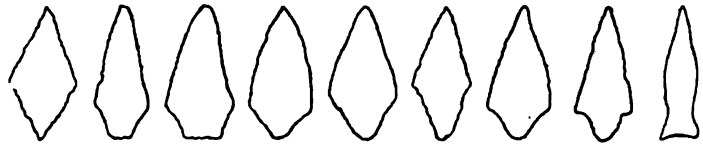


DIVISION II. TRIANGULAR.

This division includes all forms approaching a triangle, whether the bases or edges be convex, straight, or concave.

DIVISION III. STEMMED.

Class A. Lozenge-shaped.

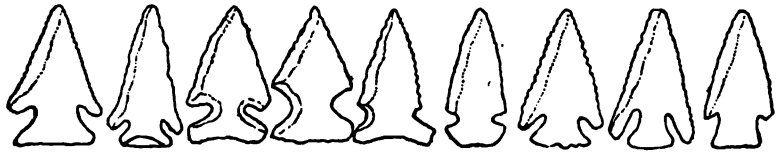


Class B. Shouldered but not barbed.

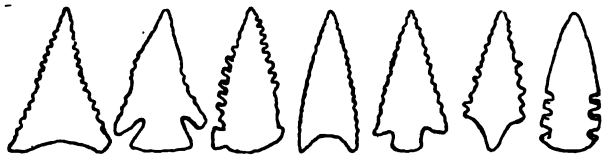
Class C. Shouldered and barbed.

DIVISION IV. PECULIAR FORMS.

Class A. Beveled edges.

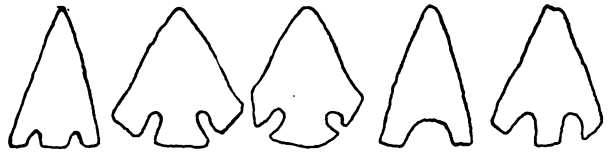


Class B. Serrated edges.

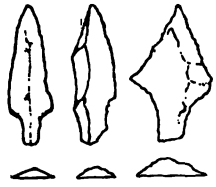


Class C. Bifurcated stems.

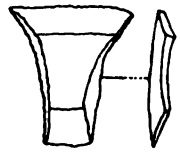
Class D. Long barbs, square at the ends.



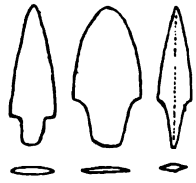
Class E. Triangular in section.



Class F. Broadest at cutting end, *tranchant transversal.*



Class G. Slate and polished.



Class H. Asymmetric.

Class I. *Forma curiosa.*

This classification is definite, certain, easily understood and remembered, and nearly unmistakable. It is believed to be valuable and, it is hoped, will be adopted into common use.

The bow and arrow was probably more of an epoch-making invention than any other. Probably no other invention had so much to do as it with the advancement of civilization. Its origin, either as respects time or place, is entirely unknown. It spread over almost the entire world in prehistoric times. Nearly every known explorer or traveller of whatever time and in whatever country found the bow and arrow in common use while its advent was unknown. The stone arrowpoint has been found in every prehistoric country. The Eastern and Western hemispheres both testify, even to their uttermost parts, the extent of its use and the almost universal occupation of the prehistoric man who made it. The bow and arrow was the earliest projectile weapon made or used by man. Before it, man had no projectile other than that thrown by the force of his own arm. The bow and arrow maintained its supremacy as a projectile machine throughout the ages, until the comparatively modern invention of gunpowder. Homer and the Siege of Troy carry our knowledge of the bow and arrow back to the eleventh or fourteenth century, B. C., and subsequent literature of classic antiquity is full of references to it. Xerxes' army, said to have been the largest ever brought together, representing nearly every known tribe or people and gathered from every country, had the bow and arrow as its only projectile machine.

An understanding of the antiquity of the bow and arrow may be approximated by a consideration of the fact that with all the spread of the stone arrowpoint, it had been superseded and given place to metal before the beginning of history. Not only in Greece and Rome, not only with Xerxes, but among the Chaldeans, Assyrians and Babylonians, and among the greatest bowmen of them all, the Scythians, the stone arrowpoint had been superseded by bronze or iron, before their respective histories began. Italy stands near the head of the list of countries of the world as to the number and fineness of stone arrows found within her borders. The most prolific spot is on the shores of Lake Trasymene, where Hannibal so successfully defeated the Roman Army. One would suppose these arrowpoints might be the remains of that battle, but we know by history what arms these armies used and how the arrowpoints were metal and not stone. Therefore, the stone arrowpoints, despite their great number and their distribution throughout almost the entire earth, had passed away and gone out of use before the beginning of history.

The favorite material for stone arrowpoints throughout prehistoric times was flint, and the first industrial art in which man engaged was the chipping of flint. This was carried to such degree of excellence, and was accomplished with such skill and dexterity amid so many difficulties, that in "Art in Prehistoric Times,"[1] I have classed it among the fine arts. Flint exists in almost every degree of fineness from coarse chert to

[1] In course of publication in Report U. S. National Museum, 1896, pp. 325-664. Pls. 74, figs. 325.

fine chalcedony, and in some of these forms it is found in almost every country of the world. Formerly flint was declared to be confined to that material composed of silica which was found in nodules in the chalk, and as the chalk formations were European and not American, it was said that the true flint did not belong to America. But a series of investigations, first by chemical analysis and second by microscopic study of thin sections, has enabled us to determine the similarity of European and American flint, both in substance and in structure.

Reference has been made to the possible use of these implements as arrowpoints, spearheads and knives, and to the difficulty in determining the special use for which the individual specimen was intended. It must be remembered that the shaft or handle, always of wood, reeds or similar soft and porous substance, has in nearly every case decayed and been destroyed. Of the many arrowpoints or spearheads found, probably not one in a million has been found with its shaft or handle intact. We are therefore driven to the consideration of the flint object alone for the decision of this question. If we could even occasionally find the shaft or handle attached, it would afford a solution of the question. The size of the point or head has but little to do with the matter. The largest flint head as well as the smallest agate point fixed in a stout shaft eight or ten feet long with a tapering end, would make a spear; and the same in a slender reed, thirty inches long, would be an arrow. A stout handle four feet long would be a javelin, while a handle of any of these kinds four or six inches long, would make it a knife.

These determinations depend entirely upon the size, weight and length of the handle and only in a slight degree upon the stone which forms the point or head. We have found in the vast collections of the U. S. National Museum many hundred unilateral or one-sided flint implements which, if regular and symmetric, would pass as the usual arrowpoint or spearhead, but in their present condition that use is impossible and they could only have been intended for and served as knives. These outlines have been elaborated in a paper just completed and now going through the press, under the title of "Arrowpoints, Spearheads or Knives."

SOME OF THE EVIDENCES THAT NORTHMEN WERE IN MASSACHUSETTS IN PRE-COLUMBIAN TIMES. By Miss CORNELIA HORSFORD, Cambridge, Mass.

THE EARTH-LODGE. By WASHINGTON MATTHEWS, Washington, D. C.

ART should be an aid to ethnography; but it is often employed in a way to deceive the student rather than to aid him. Artists are prone to make pictures to please the eye rather than to tell the truth. Works of travel are often embellished by artists who have no personal knowledge

of the scenes depicted. The author gave some instances of this misdirection of art, in general; but spoke of it especially in regard to the Indian earth-lodge. He described the earth-lodge and presented several illustrations of the lodge as it existed in former times and as it exists to-day. In the beginning of the century there were thousands of these dwellings in the Missouri valley between St. Louis and the Yellowstone and there is archæological evidence that they once extended over a much wider area. To-day there are but five or six in existence and these are on the Fort Berthold reservation in North Dakota. Morgan says of the earth-lodges that they were the most commodious aboriginal homes found in America north of New Mexico. Some were eighty and ninety feet in diameter. Each lodge consisted of a great wooden frame, covered with willows, hay and earth. The structure of the frame was explained; cross-section and ground plan were exhibited. Illustrations from the works of Catlin, De Smet, Morgan and Gass were shown and the errors of the illustrations were pointed out.

Sections F and H met at the Harvard Medical School at 8 P.M., on Tuesday, August 23, when Dr. THOMAS DWIGHT lectured upon "Variations in Human Bones," with illustrations from the collection of the Medical School.

THE SIGNIFICANCE OF THE GARMENT. By ALICE C. FLETCHER, Peabody Museum, Cambridge, Mass.

THE data from which this study of the significance of the garment, including all regalia, was based, had been taken from the religious observances and rituals, the social usages, and the individual habits of the Omaha Indians and their cognates. Many years of close personal relations with these people had led the writer to a familiarity with their customs and modes of thought enabling her to come nearer to their point of view upon various subjects than could otherwise have been possible. One of the closing paragraphs of the essay gives the writer's summary of her subject as follows:— "From a study of the Sacred Legend of the Omahas, and of the various customs and rites of the tribe and its cognates, we have found the garment to have been an invention by which man's self consciousness could be emphasized. Its practical uses were subservient to this primary purpose. We have noted that man's self consciousness was born of attrition with his fellows, that his use of garments indicated not only the birth but the development of his conscious personality, that they expressed his relations to the unseen world, that they came to characterize his experiences and exploits, to mark his place and obligations in society, and finally his freedom in personal expression."

The paper was illustrated by lantern slides, giving a unique series of pictures showing the adjustment of the robe or blanket under the impulse of passing emotions.

[The paper will be published by the Peabody Museum, Harvard University.]

RÉSUMÉ OF RECENT STUDIES ON THE ORIGINS OF EUROPEAN RACES. By Dr. W. Z. RIPLEY, Massachusetts Institute of Technology, Boston, Mass.

PRESENTATION OF A BIBLIOGRAPHY OF THE ANTHROPOLOGY AND ETHNOLOGY OF EUROPE. By Dr. W. Z. RIPLEY.

REPORT OF COMMITTEE ON THE ESTABLISHMENT OF AN ANTHROPOLOGICAL JOURNAL IN AMERICA.

REPORT OF COMMITTEE ON THE TERMINOLOGY AND DEFINITIONS OF ANTHROPOLOGY: ANTHROPOLOGICAL TERMINOLOGY, by Prof. D. G. BRINTON; SOME DEFINITIONS IN ANTHROPOLOGY, by Dr. W J McGEE.

DISCUSSION: IN MAN, WHAT FACTORS ARE DUE TO HEREDITY AND WHAT TO ENVIRONMENT? Opened by Prof. D. G. BRINTON, Dr. W J McGEE and Dr. W. Z. RIPLEY.

[Dr. Brinton's paper published in the American Anthropologist, Sept., 1898.]

ANTHROPOMETRIC INSTRUMENTS. By Prof. J. MCKEEN CATTELL, Columbia University, New York, N. Y.

Two instruments were exhibited that had been used in making tests on a large number of persons where it was desirable to avoid complicated apparatus. One was a dynamometer for measuring fatigue, the amount of each pressure of the fingers being recorded and continuously added. The other measured in a series of trials the time needed to recognize a color and make a corresponding movement.

THE NEW THEORY OF THE LIGHT-SENSE. By CHRISTINE LADD FRANKLIN, Baltimore, Md.

IT was shown in detail that the theory of the light-sense[1] which Prof. G. E. Müller proposes to substitute for that of Hering must be regarded as a new theory, and not simply as a modification of the former one.

There is no doubt that this theory must be taken as replacing that of Hering, — it is much to be preferred to it in every respect in which the two theories differ. Assimilation and dissimilation are most improbable chemical processes as the bases of the sense for the complementary colors, and they are not even, in the last analysis, in any correct sense of the term, antagonistic. When dissimilation is by any means increased, assimilation is not thereby diminished, but, on the contrary, it goes on more rapidly than before, for the sake of repairing waste. The conception which Professor Müller proposes to substitute for assimilation and dissimilation (which constitute the groundwork of Hering's theory) is that of a *reversible chemical action*. In this he is following closely upon the most recent ideas of the chemists, as set forth by Nernst and Ostwald.

The extraordinary fact of color-vision — that certain pairs of colors, not to be distinguished beforehand from any other pairs, completely disappear when seen together — is what must first be reasonably accounted for by any theory. (Such pairs of colors cannot be called either *complementary* colors, or *antagonistic* colors, without committing one's self to one or the other of the two classes of theory regarding color. They might perhaps be called, *ad interim*, the *disappearing colors*.) The existence of the "disappearing color-pairs" is well explained by the theory of Professor Müller, far better than by that of Hering. But Müller follows the worst defect of Hering in regarding black and white as due to an exactly similar retinal process with that which produces a disappearing color-pair. This defect made Hering's theory impossible of acceptance to any one with a keen sense for logical harmony; it is technically overcome, in the theory of Müller, by supposing that there is a constant production of a grey sensation in the cortex (or some other of the higher centres) and that black and white, which destroy each other in the retina when present in equal amounts, affect this cortical excitation by way of addition or subtraction, according as one or the other of their underlying processes is going on in the retina in excess. This is ingenious — the trouble with it is that it is too ingenious. There is no sufficient reason for regarding

[1] The term *light-sense* (which is formed in correspondence with *temperature-sense*) is what it has become necessary to substitute for what was formerly called color-sense. It was once believed that black and white and all the intermediate series of greys were made up out of a mixture of the sensations of color; they could then be included under the term *color-sense*. But they have now been restored to their rightful position of dignity,—they are known to be, as *sensations*, independent, and most important. All the sensations given us by light could be subsumed under *the color-sense and the grey-sense* (including the end members of the series, black and white, in the term grey), but they can be more briefly summed up as the *light-sense*.

black and white as being caused by antagonistic chemical processes — they are *not* a disappearing pair. They are exactly similar in appearance, in their intermediate stages, to any one of the *non*-antagonistic pairs, as blue and green, but not to blue and yellow. The apparent antagonism which they show in the phenomena of contrast is fictitious. Professor Müller is able to get around this difficulty, it is true, but it is better not to introduce a difficulty which exists merely for the sake of exhibiting one's skill in overcoming it. The existence of the two different types of partial color blindness can only be explained on this theory, by extremely intricate and improbable *ad hoc* hypotheses. The theory is, therefore, though it is far better than the theory of Hering, not a theory which can be accepted.

The value of a new theory by which it is proposed to account for a connected group of mysterious facts can only be properly estimated when it is contrasted with the other theories which it is proposed that it should displace. The honored Vice President of this Section has maintained for some years that the best theories of the color-sense are that of Donders, that of Wundt, and that of Ladd-Franklin; in this opinion the present speaker most heartily concurs.

THE NORSEMEN, THE CONQUERORS OF BRITAIN. By PAUL DU CHAILLU, New York, N. Y.

THE DISAPPEARANCE OF THE CLIFF DWELLERS. By DÉSIRÉ CHARNAY, Paris, France.

IS THE STRINGED MUSICAL INSTRUMENT PRE-COLUMBIAN? By Prof. EDWARD S. MORSE, Salem, Mass.

ZUNI WITCHCRAFT. By MATILDA COXE STEVENSON, Bureau of American Ethnology, Washington, D. C.

PROBLEM OF THE RECHAHECRIAN INDIANS OF VIRGINIA. By WILLIAM WALLACE TOOKER, Sag Harbor, N. Y.

[Published in the American Anthropologist, Sept., 1898.]

AGENCY OF THE MOON AMONG PRIMITIVE PEOPLES. By R. J. FLOODY, South Ashburnham, Mass.

PHYSICAL DIFFERENCES BETWEEN WHITE AND COLORED CHILDREN, OF THE SAME SEXES AND THE SAME AGES. By Dr. ALES HRDLICKA, Pathological Institute of the State Hospitals, New York, N. Y.

THE writer presents a study of the more stable differences of a physical nature which can be observed between white and black children.

The study is a result of investigation of fourteen hundred children, of whom about three hundred were negroes. The children ranged from three years of age to beyond the ages of puberty.

The differences found in the two classes of children may be arranged into those which are observed equally in both sexes, and those which are prevalent in either the boys or the girls.

DIFFERENCES WITHOUT REGARD TO THE SEX OR THE AGE OF THE CHILDREN.

The white children show more *diversity*, the negro children more *uniformity* in their normal physical characters. This fact becomes gradually more marked as we advance with the age of the children.

SIZE OF THE BODY.

The form of the head is much less variable in the colored children than it is in the American-born white children. A pure colored child shows mostly a long and narrow head, whilst the normal white American child will show us everything from a pronounced short-head to high dolichocephaly.

The forehead is in average slightly narrower in the negro child than it is in the white. Its average height, however, is not smaller.

The face of the colored children is generally more prognathic than that of white children. The prognathism is both facial and alveolar.

The malar bones are somewhat more prominent in the colored child, but the difference is not so great as that which may be observed between children of the yellow and the white races.

The nose in the negro is frequently shorter and generally somewhat lower and broader than the nose of the white child. These differences increase with the age.

The lips of the colored children are very prominent. Their mouth is broader and also more spacious antero-posteriorly than that of white children. In colored subjects the palate is longer than it is in the white and is more regular. The uvula in the colored is in the average shorter and less frequently deflected.

The teeth of the negro children are frequently stronger and healthier than are those of white children. Irregularities in the setting of the front teeth, which are so frequent in white subjects, are rather rare among the colored. The cutting of the various teeth is also more regular in the colored.

The lower jaw is somewhat higher in the negro child than it is in the average white one, and the lower maxilla is a little stronger in the former.

The ears of the negro deserve special notice. They are in average *smaller* than the ear of the whites, and they show in many cases a marked and almost specific character, which is but rarely seen in the white. This character consists in the helix being bent on itself and compressed at the highest fourth of the ear.

The body in the two classes of children shows marked differences and some of these differences are more marked in children of certain ages than they are in adolescents or in adults. These differences are much more marked in the female children than they are in the male.

In general, the body of the negro child shows less adipose tissue and relatively more muscular development. The weight of the white child is in the average at all ages slightly greater; nevertheless the negro children of all ages and both sexes are in average three to four pounds stronger in either hand than the white children of same sex and ages. The chest of the colored children is somewhat deeper.

The pelvis in the colored child is more inclined forwards than that of the white child, and this is true in both sexes. This inclination gives rise to a greater prominence of the buttocks in the negro.

The arms of the colored child are slightly longer than are those of the white, and the arm-spread of the negro is relatively greater. Both hands and feet are longer in the colored than they are in the white child.

The thighs of the negro child show remarkable difference from those of the white. They appear not unlike the thighs of a frog, being most prominent in the middle. This character is due, as the writer found by comparative measurements, to a higher curvature of the thigh bone in the colored.

The legs, as were shown by actual measurements, are somewhat smaller in the negro child than they are in the white.

The lower extremities of the colored children are regularly longer than are those of the white. Their length increases regularly in both white and colored subjects, through the whole childhood.

DIFFERENCES PECULIAR TO BOYS.

The negro boy is generally well built, lean and muscular. The body, unlike that of many normal white boys, is straight and symmetrical. The lumbar curve is more pronounced, and the buttocks are more prominent. The penis is longer than that of a corresponding white boy.

DIFFERENCES PECULIAR TO FEMALE CHILDREN.

The colored girl, before the age of puberty, and sometimes beyond this period, *is a great deal more the shape of a boy* than is the case with the white girl. Among white children, girls showing decided feminine characters, that is, feminine thorax, waist distinctly narrowed, and large hips, can be seen as early as the eighth year of life. Among negro female children these characters do not generally become manifest after twelve years or even much later.

The inclination of the pelvis in the female colored child is very apparent.

THE ORIGIN OF THE CONFEDERACY OF THE FIVE NATIONS. By CH. H. HENNING, New York, N. Y.

WHEN I recently visited the Onondaga Indian reservation, I heard from chief Daniel La Fort the following story about the origin of the confederacy of the five nations:

A little over three hundred years ago, one of our forefathers, who was an Onondaga, and whose name was Hia-wa-tha, met the Mohawk for the purpose to get married; he raised quite a family, but his wife became sick and died and he became a widower. Soon after her death he began to travel.

The law at the time when Hia-wa-tha lived did not allow to anybody to appear before the people for the space of one year and forced the person to live at a place, where he could not be seen by the people.

When Hia-wa-tha now was a widower he followed this law and held himself secretly for one year.

Furthermore it was the law at that time, that the people, that held the authority of the nation, would come to the mourning person and say: "we will enjoy together in some way." But it was not so, when Hia-wa-tha's time was expired. He also had expected, that the people would come to him and say: "we will enjoy together," but nobody came. He was patiently waiting at his secret place and said to himself: "the people do not regard me as a useful man, as I always was among the nation." Then he made up his mind, that he would appear in other places; he started entirely alone and came to Oneida. He saw there a group sitting close by a tree and asked them, "What are you doing here?" They said: "we are hiding the body of the tree, so that the wind will not strike us." And he said to them "I shall call you a large body of trees" (ni-ha-di-ān-da-go'-na).

Then he saw, travelling further, a group of people lying on the ground; he asked the same question. "We are hiding against the wind on the hill side," they answered. "I shall call you a hilly people" (Onondaga), Hia-wa-tha said.

Then he went by the Cāyuga lake and asked the people there the same question. They answered, that they lived close by the lake. I shall call you "Cāyuga Lake Indians." Then he went down at Geneva, where the Seneca Indians had a reservation and asked people the same question. They answered that they kept the door of the West; they had a kind of a fort, where they could be preserved before the wild enemies and they had a place on the western side, where they could go out and in. Then he said: I shall call you "doorkeepers." And he said, it will be the same at the eastern part of the reservation, where the Mohawks guard the entrance of the East. So the Cāyugas became the doorkeepers of the West and the Mohawks those of the East.

This was his second visit among the Mohawks.

And he left again the Mohawks and went away in a canoe; while he was paddling, he saw people close by the water and asked them, "What are

you doing there ?" They replied: "Our special business is hunting for wolf." He saw indeed many wolves lying on the ground. Then he said: "I shall call you a wolf clan." This was the first time, that the name "clan" was given, but it is unknown to which tribe the name was given. Then he said: "Whenever you build a wigwam, all of the wolf clan shall stay together and you must set up a pole, high enough that you may see the idol of a wolf on the top of the pole and so people will know, to what clan you belong."

And Hia-wa-tha went further and saw other people, peeling barks with turtles and asked them the same question. "We are peeling barks with the turtle," they replied. "So I shall call you a turtle clan," Hia-wa-tha said, "and whenever, etc."

In the same way Hia-wa-tha gave the name of a beaver clan, a bear clan, a deer clan, an eel clan, a snipe clan and a small spotted turtle clan, to the different people he met in his further voyage.

And he said then to the people: "I have finished my work in forming the different clans; you will not see me immediately again, because I have to form a law of government for the different clans." And then he went further and gathered the small shells on the banks of the river and made strings of different length and width, and built himself a wigwam and retired secretly into it.

And the people staying outside, saw Hia-wa-tha sitting in the wigwam and saw how he read the different strings, of which he had made one for each clan. And the wampums he was making were proposed to be the different laws for the different clans.

And the people went back to their tribes and told to the principal men what they had seen and heard. And the principal men said: "We must invite that man to provide a council." And they proposed that a general council should be held and informed the people that Hia-wa-tha should tell what he proposed to do.

And they selected certain men to invite Hia-wa-tha by a verbal invitation to attend the general council. But when the people came together to attend the council Hia-wa-tha did not come.

And the people sent another spy to his place to stay outside of his wigwam and to listen to what Hia-wa-tha would say. And the man heard that he complained to himself that the people had invited him only by a verbal invitation and had nothing to show. "If they would have been wise enough they should have made some string of shells and also a small stick on which the string of shells had been fastened, and on this stick they ought to have made marks to see on which day the meeting should take place."

And the man, who had listened to what Hia-wa-tha said to himself, went back to the people and told what he had heard. And they did as Hia-wa-tha said, and sent a man with a wampum stick; when he came to Hia-wa-tha he said: "This is the way I ought to be invited."

Then there was a great council meeting of all the people, and Hia-wa-tha came also, and asked them what was the proposition they had invited him for. One of the members of the council, selected as their speaker,

stood up and said: "We have heard that you are working something of a very important business, and we are very anxious to know what it is about." Hia-wa-tha said: "It is true, I am working to make if possible an organization for your people, and also for the other tribes, for the purpose to unite all the five nations." Then the speaker said: "That is true; we are at present in a state of terror, we are unsafe, and are fighting against each other. This must be stopped." Hia-wa-tha said: "If you will accept my proposition, go back with me whence I came, and I shall try to unite the minds of the five different nations and to bring them together."

And they resolved to go back with him.

And it was concluded, that at the place where the Onondagas lived, the great council of the five nations should take place. But in the first council they were not able to create a government for the five nations, and so a second council was convoked. In the second council there was concluded, that the five nations should be considered as one confederacy; but also in this second council they did not make a government for the five nations. And therefore they concluded that a third and last general council should be held to finish the entire work of the constitution.

This last general council took place where there is to-day the city of Syracuse at the place between corner of Warren street and Tennessee street.

At this last council they made as a rule to have chiefs, "a certain number for each clan."

And at that time they made wampums of bones in place of wampum of shells, "because they would be more durable," they said.

The number of the chiefs, Mohawks nine, Oneidas nine, Onondagas fourteen, Căyugas nine, Senecas eight, has been maintained up to this time.

And Hia-wa-tha said when they elected the chiefs: "There shall be one head chief for each tribe and nation. He is the only person to notify the important business to be transacted."

And Hia-wa-tha was made the head man of the head chiefs among the Mohawks.

And the Oneida elected da-dje'-tĕ as their head chief.

The Onondaga selected a very strange looking man, ta-do-da'-ho, as their head chief. For a while he lived among the Onondagas, but afterwards he disappeared. But the name ta-do-da'-ho still continued, and when a man should overtake the office of a head chief of the Onondagas, then he shall be called ta-do-da'-ho.

Then Hia-wa-tha created the position of a king of the five nations.

The Căyugas also appointed one head chief with the authority of the tribe, and called him ha-ga-ă'^{n}-yuk.

The Seneca selected two men as doorkeepers.

Then Hia-wa-tha ordered the rules of the condolence meeting by which the new chief was selected in the place of the deceased.

Then Hia-wa-tha, after having finished his great work of the organization, said to his people: "This law shall operate as long as the five na-

tions exist, and it will end, when the world will end too." And then he bade his farewell to the people and was taken up in his white canoe to the heaven.

The "Holder of the Heavens," to whom the foundation of the confederacy is also contributed, lived long before Hia-wa-tha. Between them both is quite a space. The first was a superhuman being, who destroyed the numerous monsters, but he made no constitution.

THE SURVIVAL OF AFRICAN MUSIC IN AMERICA. By Mrs. JEANNETTE ROBINSON MURPHY, New York, N. Y.

THE CEREMONIES OF THE SACRED POLE OF THE OMAHA TRIBE. By FRANCIS LA FLESCHE, Indian Bureau, Washington, D. C.

THE paper opened with a description of the Omaha tribe upon one of its annual buffalo hunts, at which time the Sacred Pole accompanied the people and was the object of reverent care. The writer was then a boy and, curiously interested in observing the old priest and his mysterious burden, he graphically detailed a purification ceremony in which he took part and which was made necessary by his accidentally touching the Sacred Pole. The great tribal religious festival of anointing the Sacred Pole was also described from memory, presenting an authentic account of a ceremony that is forever lost, and has not been observed for over twenty years. A brief history of the change that has taken place in the life of the tribe, causing the loss of the tribal rites, was given also the story of the preservation of the Sacred Pole through these vicissitudes, together with a narration of the writer's final success in 1888, when he induced the keeper of this sacred object to place it in the Peabody Museum, Harvard University, for safe keeping. After many efforts, during the past ten years, to recover the ritual of the Sacred Pole the writer finally secured it during June of this year, by aid of the graphophone. Mr. La Flesche then gave the ritual, translating it, and explaining its meaning, both by reference to the accompanying acts of the ceremony as seen by him when a boy, and by the interpretations given him by the old priest from whom the ritual was secured.

The paper, together with other information relating to the Sacred Pole, and a complete transcription of the ritual, words and music, will be published by the Peabody Museum, Harvard University.

STUDY OF NORMAL WHITE TIBIA. By Dr. ALES HRDLICKA, Pathological Institute of the New York State Hospitals, New York, N..Y.

THE study of the tibia is one of a series of investigations which the writer is carrying on, on the large collection of normal bones in the New York College of Physicians and Surgeons.

The records represent an analysis of the examination of about two thousand adult normal bones of the white people of various nations and both sexes, and they show the variations in the shape and in the size of the tibia, which may occur in the normal white subject.

The most striking thing about the normal white tibia is the number of variations to which its shape is subject. The bone is hardly ever exactly alike in any two skeletons, and it will occasionally differ to a marked degree within the same body.

The differences in the extremities of the bone are relatively of a small proportion. The variations in the shape of the shaft are very numerous; they can best be appreciated when we study the shaft in a transverse section, and especially when this section is made at the measured middle of the bone. The lower part of the tibia is, as a rule, less well defined than its middle, and the upper part of the bone is, in very many cases, disturbed in its shape by a greater or lesser overgrowth of the oblique ridge of the soleus.

Although variations in the shape of the sections of the shaft of the tibia are almost endless, yet many of these variations are only of a secondary character, and the writer is able to reduce the shapes of the bone into six principal groups or types. (Illustration of these types by plaster of Paris sections at the middle and by drawings.)

The shape of the shaft which occurs most frequently in both the white male and the female is that of a prism. About three-fifths of all tibiæ are of this variety, but the prism shows two very pronounced differences. In a certain number of cases the outline of the prism will approach more or less that of a right triangle. In the second class of prisms, the posterior surface will face backwards and outwards, and the base of the triangle is formed by the internal instead of by the posterior surface of the tibia.

Numerous cases present a transition between the two types of a prism.

The next modification of the shaft is a pronounced concavity involving the upper two-thirds of the external surface of the tibia. The most pronounced concavity is met with more frequently in the male than in the female. It affects almost generally both the tibiæ in the same subject and it reaches the highest degree in bones that show external curvature. Slight concavity of the exterior surface is quite common.

The fourth variety is a more or less quadrangular shaft. The posterior surface of the bone in these cases is divided by a vertical ridge, descending from the oblique ridge into two distinct surfaces. This type is but seldom seen in the female tibia.

The fifth type of the tibia is that where the interior border is indistinct and the posterior surface and sometimes the whole posterior half of the bone is convex or oval. This variety of the tibia is almost entirely restricted to the female sex.

The last variety of the tibiæ includes those cases where the shape of the shaft is irregularly oval. These sections, but not the whole bones, approach closely the sections obtained on the tibiæ of the gorilla. Among the

whites, tibiæ of this nature are but rare exceptions, but they are frequently met with among the negroes.

The size of the tibiæ shows also many variations.

All the measures differ very frequently not only in different subjects but also on the two sides in the same body. The left tibia is in both sexes, in average, slightly longer but to this there are many individual exceptions. The average male tibia is distinctly longer than that of the female.

The average measure of the male tibia on the right was 36.45 cm.; on the left, 36.48. In the female the right tibia measured on an average 34.5; the left 34.6.

The scale of length in the male varies from 31.0 to 45.5 cm. and in the female from 28.0 to 39.0 cm. In the male the right tibia was found longer in 42.8 per cent of the cases; the left longer in 25 per cent; equal length in 28.6 per cent of the cases. In the female the right bone was the longer in 31.8 per cent; the left in 54.5 per cent; the bones were equal in 13.6 of the cases. There are some racial variations in the length of the tibia.

Measures as to the strength of bone were taken both at the middle and at the height of the nutritive foramen.

The writer advises that a preference be always given to the measures on the determined middle of the tibia, as they were found to be by far the more stable measures. The measures at the nutritive foramen are in almost sixty per cent of cases influenced by the oblique ridge.

Another objection is that the nutritive foramen lies in various distances from the upper articular surface of the tibia. This distance differs both in different subjects and in the tibiæ of the same skeleton. In about one per cent of cases the foramen will be double.

The measures taken at the middle of the tibia show the following proportions:

The average antero-posterior diameter at this height is in the male, on the right, 3.13 cm.; on the left, 3.14 cm. In the female, the antero-posterior diameter is on an average, on the right, 2.72 cm.; on the left, 2.57 cm.

The variations in the male are from 2.6 to 3.7 cm.; in the female from 2.0 to 3.3 cm.

The lateral diameter of the tibia average, in the male, on right 2.24 cm.; on left 2.2 cm. In female, on right 2.05 cm.; on left, 1.86 cm. The variation of this measure is from 1.7 to 2.95 cm. in the male, and from 1.8 to 2.2 cm. in the female. Equal height and width of the bone on both sides in the same subject were observed with the height in 14.7 per cent of the male, and 52.4 per cent of the female, with the width in 15.4 per cent in male and 38.1 per cent of the female.

The measures at the nutritive foramen — those which are unaffected by the popliteal ridge — are generally greater than the corresponding measures at the middle. The average difference in the height is, in the male about 5 mm.; in the female about 2 mm., while the average excess of width amounts in the male to a little over 3 mm. and in the female to a little less than 2 mm. At the nutritive foramen the tibia is in average relatively somewhat higher and narrower than it is at the middle. The

width-height index at the middle in the male averages 71.1, while that at the nutritive foramen averages but 70%. The same indexes in the female are respectively, 71.9 and 69.6. Thus it may be said that the tibia, while it is in both sexes of about the same proportionate thickness, is in both sexes about 2 or 3% more platicnaemic at the nutritive foramen than it is at the middle.

Flatness of the tibia is very rare in the whites; it is also rare in the negroes.

As to the signification of the above studies, the author states the following:

The variations of tibia are much more numerous in individuals of the white races than they are in any other races in which he had an opportunity to examine the tibia. In both the pure negro and in the American Indian the shapes of the bone are fewer.

The male tibia is in the average better differentiated than that of the female. This is partly due to the greater muscular activity of the male, and partly to a persistence in the female tibia of more or less of the infantile character of the bone.

The source of the numerous variations of the shape of the tibia is not definitely known.

The considerable want of regularity in the length and in the strength of the tibia on the two sides of the body leads the writer to believe that such differences are in many instances due not to inborn conditions, but to acquired habits of the individual and also to his occupation.

THE ILLUSIONS OF CRANIOMETRY. By Dr. FRANK BAKER, Washington, D. C.

THE various metrical inventions for estimating the specific characteristics of the human skull, such as the facial angle, the occipital angle, the auricular radii and the numerous indices, have not proved satisfactory and cannot be relied on as distinctions of race. This shows that the races are zoölogical varieties not yet sufficiently fixed to admit of definite determination.

ON THE ORIGIN OF THE FAMILY. By H. SOLOTAROFF, M.D., New York, N. Y.

THE primary sexual relations among primitive man and woman issue from the bio-psychic peculiarities of the individuals to be so related. *They* — the relations — are the resultant of the definite bio-psychic peculiarities of the male and female as they have been inherited from animal ancestry.

That these sexual relations seem to be formed and changed under the constant pressure of the social sentiment which is inherent to animal life. That due to these bio-psychic peculiarities the sexual relations of the primitive male and female have been temporary, lasting a longer or a shorter period, but always contracted freely and as freely dissolved. That certain evidence in the very primitive ethnic group, as well as certain survival customs among savages of higher social development, prove this conclusion.

Moreover, certain undeniable facts in the life of the savage point to promiscuous intercourse of the sexes, but a careful study of the phenomenon would show that it has not existed as a separate state in the evolution of the family, but that it is an *incidental occurrence* in the life of the savage, resulting from the *ecstasy of play*.

[Published in the American Anthropologist, August, 1898.]

ANTHROPOLOGY, NOT SOCIOLOGY, AS AN ADEQUATE PHILOSOPHY OF HUMAN LIFE. By DANIEL FOLKMAR, Milwaukee, Wis.

SOCIOLOGY is proving to be a disappointment, now that it has been given a place largely in American universities. Its doctrine of the social organism leads to socialism. It overlooks the importance of individual activities. Recent French and Russian writers have practically settled this question. They have shown that "social phenomena" can be reduced to the psychical phenomena of individuals. The "social organism" is an analogy, a fiction.

The "new anthropology," now being developed by the Americans Brinton, Powell, McGee and others, bids fair to become a broader and more satisfactory philosophy of human life than sociology can ever be. It is much broader than the old, physical anthropology of Europe. The chief addition that must be made is "applied anthropology," or the conclusions of anthropology brought to bear upon social, political and ethical questions. This would provide a scientific basis for a new or positive ethics as the most important undertaking of the scholar in the present demoralization and growing disbelief in religious, ethical, political amd social principles and institutions.

So broad a study of man as this, including all the questions and problems of human life, might well be called philosophical anthropology. It would include the art as well as the science of human life, the study of the individual as well as of the society and the race. It would include the questions of the nature of man, his relation to his environment, his evolution and his destiny. To determine what should be done in any of the affairs of the immediate present, as well as what should be aimed at as the distant goal, we must call in the argument from evolution ("judge the future by the past"), the argument from present conditions, as based upon

a knowledge of human nature and of social conditions, and lastly the argument from general philosophy and all related studies as to what is possible and right.

The speaker illustrated his view of the scope of philosophical anthropology by a diagram of a cube. On its face were the following divisions or studies of "anthropo-statics," or the study of coëxisting social and human phenomena functions and activities: nutrition, reproduction, protection, innervation, esthetic function, education, communication, ceremony, religion, government — all originating in physiological functions, then becoming artificial: structures, body, mind, social groups, races, nations. The depth of this cube represented "anthropo-dynamics," or the study of the progress, evolution, or history of these phases of human life. It includes a study of the past (origins, archæology, history), of the present (statistics, ethnography, political geography), and of the future (ethics). The existing political and social sciences were shown to correspond to divisions of this scheme, but to be by their very nature fragmentary and misleading unless brought into relation with the rest. Anthropology, as thus studied, is the most difficult, as well as the most important of all studies.

MUSEUM PRESENTATION OF ANTHROPOLOGY. By W. H. HOLMES, U. S. National Museum, Washington, D. C.

I.

SCIENCE aims to examine, classify and elucidate the whole domain of man and his activities; to determine the significance of every fact and every group of facts, and to give the race the benefit of the knowledge gained. It seeks to discover the laws governing development and to use these in directing the present and in molding the future of the race.

II.

The museum is one of the most important agencies enlisted by Science in this vast and ennobling work. It is necessary, therefore, that its functions be defined and its methods perfected. Science deals systematically with all phenomena, material and non-material, but the Museum is confined in its operations to the use of material things alone. The Anthropological Museum collects, preserves, classifies, elucidates by research, describes, publishes, and exhibits the material phenomena of the race, and seeks by the use of these phenomena to assist the various other agencies of culture in laying the foundation of enlightened knowledge upon which the superstructure of the future of mankind is to be erected. Collection, preservation, investigation and exhibition are all essential features of the museum, but all these functions save the last are as well subserved by the storehouse and laboratory. It is exhibition, therefore, that characterizes the institution called a museum. The purpose of ex-

hibition is to educate by presenting the ripened results of research to the popular mind in a way at once forcible, attractive and comprehensive.

It is the place then of those who manage the affairs of the museum to decide what shall be presented in the exhibition series, and how it shall be presented. These are the vital questions of the museum.

III.

If the museum were to attempt to teach the science of Anthropology systematically by means of its exhibits it would find itself in a maze of difficulties. The limited and fragmentary collections, such as our museums usually possess, would render such an attempt abortive, and, even with a very extensive series of objects, many of the branches of anthropology could not be effectively presented. The reason for this is apparent; some of the branches are without objective representation, save of the most meagre and remote kind, as the following brief review will show:—

Utilizing objective material we may present (1) *physical man* fully and satisfactorily; we may illustrate his career individually and racially; his affinities, his embryology, anatomy, physiology and pathology. We may present (2) *psychical man*, but the mind being non-material cannot itself be illustrated in the museum; yet its character, powers, and activities may be forcibly represented by a proper assemblage of the fruits of its activities. The human activities, physical and mental, give rise to a great group of phenomena which we may denominate *culture phenomena*. These have been studied and somewhat fully analyzed by anthropologists. They are roughly separated into philologic, sociologic, sophiologic and technologic phenomena. Glancing over these we find that the phenomena of *language* can be illustrated in small part only. A display of the organs of speech, of the expressions of the features, of gestures, and of pictorial and phonetic signs assist the museum in telling the story of language and its uses. The phenomena of *sociology* are even less available for the museum, and *opinions* are tangible only through the material agencies of their promotion and the peculiar forms of art to which they give rise; but *technology* is capable of endless elaboration serving to elucidate its own varied activities as well as to aid in illustrating almost every other branch of human effort.

It is thus seen that anthropology cannot be systematically presented in the museum as it is presented by science, and we are compelled to strike out on new lines seeking points of view from which the great facts and laws of the subject may be best presented with the peculiar resources and mechanism of the museum.

IV.

A most comprehensive general idea of the human race is obtained by a survey of the tribes and nations of the world as they exist to-day These groups of people occupy the more congenial areas of the earth, the so-called characterization areas, fulfilling the functions of biological exist-

ence and carrying out their varied activities according to capacity, status of culture and environment. A clear notion of these phenomena prepares the way for a study of the more obscure and vital questions of anthropology, and the general Anthropologic Museum cannot do better, as a first step, than to assemble its materials on the geographic or ethnographic plan. The museum thus becomes a miniature world in which the visitor may pass from people to people learning of their characteristics, their arts, industries, habits, customs, etc., and pursuing more special investigations according to inclination. That this presentation should be effective it must show by means of models and pictorial representations a type people of each group or region. There should be men, women and children engaged in characteristic occupations, and surrounded by exhibits illustrating all phases of their culture and environment.

By this method of presentation we teach (1) of tribes and nations and their characteristics and distinctions; we teach (2) of culture and its very suggestive gradation from the lowest savagery to the highest civilization; we teach (3) of the relation of these phenomena to environment; and by assembling the antiquities of each area in the same place we teach (4) of the succession of peoples and culture, and convey a general notion of mutations of fortune and the slow progress from lower to higher phases of existence.

V.

The importance of teaching what the peoples of the world are and have been is surpassed only by the importance of determining how the race became what it is, and the methods and laws of its becoming. When collections permit of it, a second group of exhibits should be assembled to teach of man and his activities from the point of view of genesis. This work is far more difficult of accomplishment than the grouping of phenomena by areas, as the materials employed must be carefully selected and presented to illustrate the most advanced conceptions of human evolution.

These exhibits may be conveniently assembled in two groups: one dealing with *physical man* and utilizing biological phenomena; the other with the *human intellect* and employing the products of human activity in telling the story.

The exhibit illustrating *physical man* necessarily consists largely of materials derived from living peoples— the man of to-day. The origin of the race dates far back in the geological ages, but geological formations have yielded little material bearing upon the evolution of the genus Homo, so that embryology and anatomy are called in to demonstrate our relationships with the rest of animate creation, thus working out the marvellous story. More interesting and instructive exhibits cannot be conceived of than those brought together by the embryologist and anatomist who, dealing with phenomena of the present, tell the story of the ages.

The *psychological history* of man runs parallel with the physical history, but the human animal was well developed before the mind acquired its wider grasp and before its creations, the artifacts of primitive peoples,

furnished the means of recording its progress. Intangible as the mind is, its activities have filled the world with wonderful creations which record the progress of its evolution step by step, and the noblest function of the anthropologic museum is that of presenting to the world by means of these things, the story of the intellectual development of the race.

Culture has advanced along many lines, and we are able to illustrate these lines with a fulness depending upon the extent to which they gave rise to, or were accompanied by material phenomena. Owing to the extent of the field and the vast resources for display, the series of objects illustrating a single line of progress must be limited in number and must indicate the main steps only, thus presenting an easily read and convenient synopsis. These exhibits may show, for example, the acquiring arts — hunting, fishing, agriculture, mining, etc.— began in hesitating steps, employing the simplest devices, and developed by a succession of improvements to their present high degree of efficiency. They may present the arts of transportation beginning with the human burden-bearer and ending with the wonderful machinery of to-day triumphing over land and sea. They may present one hundred or five hundred branches of art and industry, all arranged to tell an eloquent story of material progress and, more especially, of the steady growth and conquests of the human mind.

VI.

The two methods of presentation referred to, the *geographic* and the *developmental* are, perhaps, the only methods that can be said to have general application to the whole field of anthropology, but the museum is not limited to these methods. Special exhibits may be arranged to elucidate limited portions of the field-portions that may have special interest, local or otherwise, or that cannot be sufficiently developed within either of the general schemes. Such may be designated *special* exhibits. Thus we may bring together a series of collections illustrating the history of the United States arranged on a chronologic basis; a collection of materia medica, assembled on the cyclopedic plan; an assortment of paintings arranged for comparison of schools of design; a set of experimental contrivance employed in developing the X ray; a series of personal relics relating to Napoleon, etc., etc.

It is thus seen that anthropology may be presented: (1) by a great group of exhibits illustrating the peoples of the world, arranged geographically; (2) by series of exhibits illustrating the evolution of man, physically and mentally; and (3) by exhibits illustrating subjects of special interest or importance from any part of the field.

The general anthropologic museum, to which reference is here made covers a distinct field from that occupied by the *industrial* or *commercial* museum. The functions of the one are scientific and educational, the functions of the other are purely practical having in view immediate material benefit. The operations and objects of the two kinds of museums being distinct they require independent organization and management.

THE HARMONIC STRUCTURE OF INDIAN SONGS. By Prof. J. C. FILLMORE, Pomona College, Claremont, Cal.

SOPHIOLOGY, OR THE SCIENCE OF THE EVOLUTION OF OPINION. By Major J. W. POWELL, Bureau of American Ethnology, Washington, D. C.

ANTHROPOLOGICAL PROBLEMS OF THE PACIFIC SLOPE. By Dr. W. H. HOLMES, U. S. National Museum, Washington, D. C.

SUBJECTS RELATING TO THE PHYSICAL ANTHROPOLOGY OF NORTH AMERICAN INDIANS. By Dr. G. A. DORSEY, Field Columbian Museum, Chicago, Ill.

THE MAORI OF NEW ZEALAND; HIS HISTORY AND COUNTRY. By Hon. HUGH H. LUSK, England.

THE SWASTICA AND OTHER MARKS AMONG THE EASTERN ALGONKINS, A PRELIMINARY STUDY. By WILLIAM WALLACE TOOKER, Sag Harbor, N. Y.

THE WATER BURIAL TIME. By STANSBURY HAGAR, New York, N. Y.

THE RITE OF CIRCUMCISION AMONG THE EARLY RACES. By R. J. FLOODY, South Ashburnham, Mass.

SCIENCE THE BASIS OF MORALS. By M. A. CLANCEY, Washington, D. C.

TYPICAL AMERICAN STUDENTS, ILLUSTRATED BY CHARTS AND STATURE. By Dr. D. A. SARGENT, Harvard University, Cambridge, Mass.

A NEW KYMOGRAPHION; A NEW CHRONOSCOPE. By Prof. G. W. FITZ, Harvard University, Cambridge, Mass.

PSYCHOLOGY AND ART. By Prof. HUGO MÜNSTERBERG, Harvard University, Cambridge, Mass.

SOCIAL ORGANIZATION AND LAWS OF THE MOKI INDIANS. By WALTER HOUGH, U. S. National Museum, Washington, D. C.

KOREAN CLAN ORGANIZATION. By WALTER HOUGH, U. S. National Museum, Washington, D. C.

THE OTOMIES OF MEXICO. By Prof. FREDERICK STARR, University of Chicago.

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SOCIAL AND ECONOMIC SCIENCE.

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ADDRESS

BY

ARCHIBALD BLUE,

VICE PRESIDENT, AND CHAIRMAN OF SECTION I.

THE HISTORIC METHOD IN ECONOMICS.

ALTHOUGH the subject of my address is the Historic Method in Economics, it seems fitting that at this meeting, which rounds off fifty years in the life of the American Association, a brief time should be spent in looking backward and taking note of some of the landmarks and milestones that have been passed on the way. If to those who follow after righteousness it is good counsel to look unto the rock whence they were hewn, so also it is wise counsel to the scholars of science, for this, too, is of the historic method. There are many things to be learned from a survey of our own experience, not the least valuable of which are the slowness of the march and the unsureness of the footing. "I have devoted my whole life to the study of Nature," Louis Agassiz said in summing up the result of his life's work, "and yet a single sentence may express all that I have done. I have shown that there is a correspondence between the succession of Fishes in geological times and the different stages of their growth in the egg, — this is all." And that being all in a student and man of genius like Agassiz, to whom a physical fact was as sacred as a moral principle, and who found in Nature the teacher which brings us back to absolute truth as often as we wander, what wonder if the pathway of science is beset with many hindrances, and if advancement in it is uncertain and slow! We expend more effort and time in unlearning what is false than in acquiring what is true,

and what all the men of science now living upon the earth do not know would fill a very large book.

When Freeman, the historian, came to America seventeen years ago, he read a series of eloquent papers before the Lowell Institute in this city, on the English People in its Three Homes. In the true historic map of the English folk, the Old England is on the mainland of Europe, and between it and the New England on the mainland of America is the Middle England, in the Isle of Britain, the child of the one, and the parent of the other. And in founding the Kingdom of England, Freeman said, those stout English folk who came over from the forests of Germany in the keels of Hengist and Cerdic founded the confederation of the United States of America and all that that name implies. But we owe them more than origin and the beginnings of free institutions. All the goodness of the old stock was not taken away by those who left the old home, and when in the fulness of time religious freedom was won the race was ready for the revival of learning, the pursuit of knowledge, and the conquest of the forces of Nature. History has been repeating itself.

As the American Association has had for its model the British Association, so the British had for its forerunner the German; and in all three there was a federation of older societies.

The German Union of Natural Philosophers was founded at Leipsic in 1822 by Dr. Lorenz Oken; it survives under the name of the German Association of Naturalists; and it has held meetings every year since the first formation, save in 1848, when there was revolution in Prussia, in 1866, when there was war with Austria, and in 1870, when there was war with France. The objects of the association, however, were not wholly scientific, for Oken, besides being a teacher in medicine and natural history, was a politician, and for criticising the Government his professorship at Jena was taken away. Three years later, when the association was formed, the political unity of Germany became one of its purposes, which, however, did not ripen until well nigh fifty years. Of Oken himself it may also be observed that the general character of his writings has not been acceptable to scientific men. He cultivated speculation too much at the expense of observation, and in this respect was not unlike one of the schools of political economists with which I purpose to deal presently.

Sixty-seven years ago there was organized in the City of York,

England, by a group of men of whom Brewster, Murchison and Whewell may be named as the foremost, a society whose aim and purpose were distinctively expressed in its name — the British Association for the Advancement of Science. In expounding the scheme of it at the first meeting, Rev. William Vernon Harcourt said it owed its origin to some distinguished cultivators of science then present, who were of opinion that great advantage might be expected from an association for scientific intercourse in their own country, formed upon the model of that which had subsisted in Germany for several years — an association which appeared "to have answered the hopes of its founders, as well in approximating men of science to each other and promoting among them friendly feelings and an instructive interchange of ideas as in giving to their union a collective efficacy and bringing their aims and views more prominently into public notice." To give a stronger impulse and a more systematic direction to scientific inquiry, to promote the intercourse of those who cultivate science in different parts of the British Empire with one another and with foreign philosophers, to obtain a more general attention to the objects of science and a removal of any disadvantages of a public kind which impede its progress, — these were the aims of the association as set forth in its constitution. And in view of the professed care to eschew politics which has more than once found expression in the proceedings of the association, it may be remarked that the last-named object was purely political. "We all know that the laws of this country," Viscount Milton, the President, said in his opening address, "I mean in particular the fiscal laws of this country, offer numerous obstacles to scientific improvements;" and he instanced the serious obstacles to the science of optics resulting from the regulations relative to the manufacture of glass. But the British Association has never permitted itself to be used as an instrument of party politics, and in all the years of its existence its work has been carried on with splendid vigor and judgment. From its inception until now the membership has embraced the best representatives in every department of science in the British islands and in the colonies. To indicate only a few, there are on the rolls the names of Professor Sedgwick and Henry Hallam, Faraday and Owen, Darwin and Huxley, Tyndall and Sir Henry Bessemer, Lord Rayleigh and Sir William Siemens. That it has been allowed to suffer no decline,

those of you who attended the meeting at Toronto last year, and who came in touch with men of world-wide fame like Lord Kelvin and Lord Lister, Prof. Michael Foster and Professor Ramsay, Sir John Evans and James Bryce, I am sure will willingly allow. Some of the association's members have been ennobled by their Sovereign for services rendered to science, and some have been ennobled by right of a diviner creation. It is, I believe, the best and longest roll of great men that the records of science and learning afford in any age or in any land.

The formation of a statistical section of the British Association was authorized at the third meeting, held at Cambridge in 1833, amidst some doubt as to the propriety of it. But it was justified by the President, Professor Sedgwick, in so far as statistical enquiries have to do with matters of fact, with mere abstractions and with numerical results. Considered in that light, he reasoned, they give what may be called the raw material to political economy and political philosophy, and by their help the lasting foundations of those sciences might perhaps be ultimately laid. "In physical truth," Professor Sedgwick said, "whatever may be our differences of opinion, there is an ultimate appeal to experiment and observation, against which passion and prejudice have not a single plea to urge. But in moral and political reasoning we have ever to do with questions in which the waywardness of man's will and the turbulence of man's passions are among the strongest elements." Therefore he feared that if the true boundaries of science were transgressed and if the door of communication with the dreary world of politics were opened, the foul demon of discord might find his way into their Eden of philosophy. But with this warning, and with the limitation that inquiries should be restricted to facts relating to communities of men which are capable of being expressed by numbers, and which gave promise when sufficiently multiplied to indicate general laws, the section was formed and its first meeting, with Charles Babbage as President, was held in 1834, the same year in which the Statistical Society of London was organized.

At the Cheltenham meeting in 1856 the name of the section was changed to the section of Economic Science and Statistics. Lord Stanley, the late Earl of Derby, who was President that year, introduced the innovation of an opening address, in which he urged the establishing of a statistical department of Govern-

ment charged with the annual publication of such facts relative to the management of national affairs as are reducible to numerical expression. "We have statistics enough presented to Parliament every session," he said, "but they are in the great majority of cases called for by individuals. They are drawn out to suit the particular purpose of those who call for them; they are accordingly deficient in unity, and often of no use beyond the moment." The scheme was soon after adopted by the British Parliament, and it has been followed in a number of other countries, including the United States, with the result that we have now available a vast mass of statistics for the study of public affairs, and in particular of questions bearing upon trade, commerce and finance.

One other illustration of the work done in this section of the British Association deserves mention at the present time, when the idea is on the eve of taking shape as a reality. At the 1858 meeting in Leeds, forty years ago, Mrs. William Fison read a paper on the Importance of a Colonial Penny Postage, viewed in relation to the Advancement of Science and Christian Civilization. In the prosecution of scientific research, the writer pointed out, unrestricted communication between men of science is an important condition of progress. "It would be difficult to estimate," she said, "the bearing a penny postage to Canada would have upon our Anglo-Saxon brethren in America. Its action upon the United States would be immediate and powerful, and most probably would quickly induce them to arrange a similar rate of postage to England. Who can calculate the beneficent results to the human race of such an intercourse between the two great Protestant nations of the world, both sprung from one parent stock, and destined probably to be again united in carrying out the designs of an overruling Providence for the Christian civilization of the world?" The beginning of another year it is hoped will see penny postage in operation throughout nearly the whole extent of the British Empire, and it is not long to wait until we can judge of the policy by its fruits — by the degree to which it will serve as the means of keeping up intercourse between distant peoples, upon which the conduct of business affairs as well as of mutual knowledge and sympathy so largely depend. In the light of existing relations, both imperial and international, the outlook is vastly brighter now than it seemed to be forty years ago. As

between Canada and the United States, only a step is required to complete the system, for already two cents will carry a letter from any part of the latter country to any part of the former, and three cents will carry a letter from any part of the former to any part of the latter — over regions nearly 8,000,000 square miles in extent.

In the standing and reputation of its officers, many of whom have occupied high places in the public life of the country, the section has been singularly fortunate, and still more, perhaps, in the complexion of its membership. At the sixty-four meetings which it has held nearly 1,400 papers have been read, a very large proportion of which were contributed by business men, and which partook in subject and treatment of the nature of business affairs. Nor have there been wanting the professional economists, among whom may be named Senior, Cairnes, Fawcett, Rogers, Jevons, Levi, Leslie, Ingram, Marshall, Sidgwick, Mavor, Cunningham, Palgrave and Gonner. And what greatly facilitates the discussion of economic subjects there — in contrast with the embarrassing situation sometimes presented at scientific meetings in the United States and Canada — is the circumstance that the trade question has been lifted out of politics. A man may speak the thing he will.

In referring to the organization and work of our own Association, I assume you are all so familiar with its record that the merest outline will suffice for the purpose of this address.

The first step to form it was taken at the session of the Association of American Geologists held at Boston in September, 1847, when a proposal was adopted whereby that body agreed to resolve itself into the American Association for the Advancement of Science, and to hold its first meeting under the new organization at Philadelphia in September of the following year. There, accordingly, fifty years ago, the constitution was adopted and the Association was ushered into life.

The name of Prof. W. B. Rogers stands first in the list of officers; and referring to the movement when addressing the Association in this hall as Chairman of the local committee at the meeting in 1880, Professor Rogers spoke of the British Association as the one "which we are to regard as the parent institution from which we have sprung." Besides himself, there were in the Association from its beginning such well-known masters of science

as Louis Agassiz, fresh from his native Switzerland, James D. Dana, Joseph Henry, Asa Gray, President Hitchcock and the elder Silliman, described by Agassiz as the patriarch of science in America.

Meetings continued to be held annually down to 1860,[1] and the next year's meeting was arranged to be held at Nashville, Tennessee, in April. But meantime the Civil War broke out, and an interruption occurred which continued until 1866.

In 1880, at the first meeting of the Association held in Boston, it was determined that the time had come when it should open the doors still wider, and by the addition of other departments of research give a broader scope to its objects. Accordingly a committee was appointed to revise the constitution, and in its report, approved by the standing committee, provision was made for nine sections, instead of two, with a number of sub-sections as formerly. The amendments were adopted at the Cincinnati meeting of next year, on the 20th of August, and then for the first time economic science and statistics got a recognized standing in the Association.[2] The first meeting was held at Montreal in 1882, and in the sixteen meetings of the section about 250 papers have been read. Among these I have observed only one whose author is a professional economist. A few of the contributors have been business men, but I believe that a large majority have been, like myself, attached to the civil service. This is unfortunate, perhaps, as men of our class are supposed to hold no opinions, especially on economic subjects when the trade question is immersed in politics. But I think a new departure is desirable, and that we should aim at drawing into the section a better representation of business men and public men and professional men, who will discuss questions of economic interest in a scientific spirit, without fear of criticism from any quarter, or by any authority. There is a grand motto for us in the prayer of the mariner of old when in a great tempest he said to Neptune, god of the sea: "O god, thou mayest save me if thou wilt, and if thou wilt thou mayest destroy me; but whether or no, I will steer my rudder true." There are some things more to a free

[1] In 1850 two meetings were held, one at Charleston, S. C., in March, and one at New Haven, Conn., in August.

[2] In 1894, at the Brooklyn meeting, and again in 1895, at the Springfield meeting, it was proposed to change the name of the section to Sociology, but an amendment to Social and Economic Science was carried instead.

man than is place, or office, or life; and one of these is the right to follow his intellect.

The subjects of papers read in the economic sections of the British and American Associations have usually appertained to the industrial life of the human race under varying conditions and circumstances, and the method of treatment has largely partaken of the concrete form. This was the case almost without exception in the first quarter of a century of the British Association, when indeed the only method allowed was the statistical. But ever since the enlargement of the scope of the section in 1856 the abstract or deductive method has had its exponents, and at least upon one occasion was ably represented by a President of the section — by Prof. Henry Sidgwick at the Aberdeen meeting in 1885.

Personally, I favor the historical method of investigating economic subjects, which includes the use of statistics, because I find it easier to work that way. But I do not say it is the best for every one, for I think something depends on the mental bias. The man with a strong turn for philosophy prefers to study, criticise and develop theories rather than to collect and systematize facts.

James Bryce, in his recent book on the Characteristics of Gladstone, says of Scotsmen in general, as well as of Gladstone in particular, that they are more fond of abstract intellectual effort than are Englishmen. It is not merely that the taste for metaphysical theology is commoner with them, but they have a stronger relish for general principles. They like to set out by ascertaining and defining such principles, and then to pursue a series of logical deductions from them. They are less content to remain in the region of concrete facts, and more eager to hasten on to the process of working out a body of speculative doctrines. The Englishman is apt to think that he is right in spite of logic; the Scotsman delights to think that it is through logic he has reached his conclusions and that he can by logic defend them.

Now, if Mr. Bryce is right in this estimate of Scotsmen, we may find in it a clue to the prevalence of the deductive method among the British economists, for the two who are first among them — Adam Smith and John Mill — were both Scotsmen. But it is not strictly true of either Mill or Smith, nor of British economists in general, as asserted by many German and some English writers, that they have treated political economy as a body of

universal truths or natural laws, or as a science whose fundamental principles are all fully ascertained and indisputable, and which has nearly reached perfection. It was not in that light assuredly that Mill regarded Smith, whose great work on the subject he has referred to as "in many parts obsolete and in all imperfect," although he admits that it invariably associates the principles of the subject with their applications.

In the chapter on wages and profit in different employments, in the first book of his Wealth of Nations, and in the chapter on the principle of the mercantile system, in the fourth book, it is plain to be seen that Adam Smith was familiar with a great variety of economic facts, and those who read him intelligently cannot fail to perceive that in a rare degree he possessed the gift to analyze and value them. Mr. Cliffe Leslie, who himself studied political economy in the light of history, and who had shared with Rogers and Thornton the distinction of being excepted from Roscher's unfavorable judgment on the English economists, knew better than his German master what Adam Smith's method really was. "The principal merit of Adam Smith's economic philosophy," he wrote in 1875, "has been generally overlooked. He combines the historical method of Montesquieu with the theory of natural law, and although that theory, together with his theological system, gave a bias to his inductive study of the real order of social progress, he has a true title to be regarded as the founder of the historical method in political economy, in the sense, at least, of having been the first to apply it." Similar testimony is borne by Professor Ingram, who dealt with the whole subject of methods in the luminous address delivered to the economic section of the British Association at Dublin in 1878. "That the master tendency of Smith's intellect was the deductive," Professor Ingram said, "or that it is at the deductive point of view that he habitually places himself, seems to me plainly at variance with fact. Open his book anywhere and read a few pages; then do the same with Ricardo's principal work, and observe the difference of the impression produced. Under the guidance of Ricardo you are constantly, not without misgivings, following certain abstract assumptions to their logical results. In Smith, you feel yourself in contact with real life, observing the human acts and their consequences by the light of experience. Of course deduction is not wanting, but it is in the way of verification.

The facts are interpreted from the nature and circumstances of men in general, or particular groups of men. Sagacious observation and shrewd comment go hand in hand." Even Ricardo himself, as Prof. Simon Patten has aptly observed, was a practical economist in the world as he knew it. He thought and wrote as a business man and stock broker, who had an intimate knowledge of the commercial usages of a great city at a time when political power was passing from the country to the town, and "as he thought only of profits, it is natural that he should see that the rise of rent reduced profits, and that profits varied inversely with wages." His laws are the outcome of those ideas which control the trading world. Adam Smith was subject to no influence of this kind, for he does not seem to have had any fitness for business. But he created the science of political economy, and, as noted by Walter Bagehot, his teaching on one side created Cobden and Bright, and rendered possible Ricardo and Mill on another. So much theory and so much practice, Bagehot said in writing of Adam Smith the man, have rarely, perhaps never, sprung from a single mind.

Concerning John Mill, we have his own statement that the Principles of Political Economy was written with a practical and popular object, but without an attempt to gain either of those advantages by the sacrifice of strict scientific reasoning; and though desiring that his treatise should be more than a mere exposition of abstract doctrines, he was also desirous that such an exposition should be found in it. In the second book, which treats of the distribution of wealth, the historical method has been largely pursued, but especially in the chapters on peasant properties. In every department of human affairs, Mill wrote in the first sentence of his book, practice long precedes science; and it was a settled part of his philosophy that deductive science is a system of corollaries from experimental science. In the System of Logic, first published in 1843—the same year in which Roscher's Outline of Lectures on Political Economy following the Historical Method was published—he took what many at the present day would regard as an extreme position on the value of the historic method as the one whereby the derivative laws of social order and progress must be sought. "By its aid," Mill affirmed, "we may hereafter succeed not only in looking forward into the future history of the human race, but in determining

what artificial means may be used, and to what extent, to accelerate the natural progress in so far as it is beneficial; to compensate for whatever may be its inherent inconveniences or disadvantages; and to guard against the dangers or accidents to which our species is exposed from the necessary incidents of its progression." I am sure that neither Dr. Gustav Cohn nor any other of the ardent admirers of Roscher could claim a higher merit for the method than these words affirm; and there is far more of the method in Mill's Principles of Political Economy than the younger generation of German critics appears willing to recognize, although from the very nature of the book it did not readily admit of specific treatment in that way.

Professor Böhm-Bawerk of Austria, a few years ago, made a very able and temperate reply to attacks upon the deductive method by disciples of Roscher, and notably by Gustav Schmoller, an editor and writer of some note. It is the opinion of Böhm-Bawerk that an extension of the empirical material of the science of political economy is advantageous to its development, but he intimates that to extract a general theory from it there are required that measure of abstraction and use of deduction which men of the historical school definitely reject. All the elements for the solution of a problem may be in the first million of instances, perhaps in the first thousand, as surely as in the last, and the art is merely to set them free; to which end it is not greater extension but greater profundity that is needed. It is not a greater accumulation of observed cases that will help, but a deeper insight into them. Both methods have their uses, Böhm-Bawerk recognized, one in the province of theory and the other in that of practical results. "In the province of practical social-politics for technical reasons, the historical-statistical method is so unquestionably superior," he confesses, "that I do not hesitate to declare that a purely abstract-deductive legislative policy in economic and social matters would be as much of an abomination to me as to others." Yet he does not fail to assert the value of the method of his own school, nor to point out that while writers of the historic school in Germany have discarded abstract deduction they cannot write three pages on economic questions even in the his toric style without general theory. Obviously Böhm-Bawerk resents the "Made in Germany" idea in matters of scientific investigation, but he is too large minded to object to any method

that is useful in the pursuit of truth. "I desire no battle between methods," he says; "I desire rather their reconciliation and fruitful coöperation: I would awaken the feeling not only that there is room for us all in the vast field of political economy, but that there is need of us all, with all our variety of talents and methods; that, instead of turning our intellectual weapons against one another and wilfully refusing to use a better way than our own when it is offered us, we should unite our forces against the common enemy, the untold army of problems of one of the most difficult of sciences." There has indeed been a keen controversy waged on the merits of economic methods during the last quarter of a century, and one who is not himself a professional economist may venture to express surprise that so much feeling and prejudice and misrepresentation should be exhibited in the discussion of what, after all is said, is only the best and surest way of getting at the truth of the matter.

Bacon, whose name is inseparably associated with the inductive method, certainly meant that all sciences, including ethics and politics, should be developed by it. There would be found some things in the experiments themselves, he had no doubt, which were not quite certain, or perhaps were quite false, and which might make a man think that the foundations and principles upon which his discoveries rested were doubtful and mistaken. But such things must needs happen at first. To his mind it was only like the occurrence of a letter or two misplaced in a written or printed page, to be easily corrected by the sense. "So, likewise, may there occur in my natural history many experiments which are mistaken and falsely set down, and they will presently by the discovery of causes and axioms be easily expunged and rejected;" Bacon said in his Aphorisms. And he implies that if in natural history there is a risk of errors in making experiments and recording observations, how much more in philosophy and sciences built on such a sand! "The object of the natural history which I propose is not so much to delight with variety of matter or to help with present use of experiments as to give light to the discovery of causes and supply a suckling philosophy with its first food. For though it be true that I am principally in pursuit of works and the active department of sciences, yet I wait for harvest time, and do not attempt to mow the moss or to reap the green corn. For I well know that axioms once rightly discovered

will carry whole troops of works along with them, and produce them not here and there one, but in clusters. And that unseasonable and puerile hurry to snatch by way of earnest at the first works which come within reach I utterly condemn and reject as an Atalanta's apple that hinders the race." Such was the office of natural history, as Bacon planned it in his mind; but why he did not mow the moss or reap the green corn is made clear by the group of pleasing similes used to illustrate the difference between the true scientist on one side and the men of experiment and the men of dogma on the other. "The men of experiment are like the ant; they only collect and use: the reasoners resemble spiders, who make cobwebs out of their own substance. But the bee takes a middle course; it gathers its material from the flowers of the garden and of the field, but transforms and digests it by a power of its own. Not unlike this is the true business of philosophy, for it neither relies solely nor chiefly on the powers of the mind, nor does it take the matter which it gathers from natural history and mechanical experiments and lay it up in the memory whole, as it finds it, but lays it up in the understanding, altered and digested." Therefore, from a closer and purer league between experiment and reason, Bacon saw that much might be hoped.

But in applying Bacon's method to the study of social and political problems, a new element comes in. The laws of physical nature possess fixity and uniformity under almost every possible and conceivable condition, whereas the laws of human society are modified by the forms of human nature and impulses of the human will, as developed under every influence to which men and nations are subject in relation to each other and to every past and present condition. It has been said that examples prove nothing where persons, times and other circumstances differ. But the differences are not always radical, and very rarely is the old life stamped out of a people. The English of to-day have in a large degree the same traits of character as they possessed five centuries ago; so also have the French and the Germans. The Arabs of the East have the same manners of life as the sons of Ishmael in the days of Job, who went forth to work as wild asses and reaped the wilderness for their children. And the Hebrews, after enduring ages of banishment from their own land and centuries of injustice and cruelty throughout every country of Europe, attest wherever a chance is given them in business affairs the abounding

prepotency of Jacob, the Syrian patriarch of nearly four millenniums ago. Change of some kind there must be: it lies, as Carlyle has expressed it, in the very essence of our lot and life in this world. But there is a more hopeful view, which Carlyle has thrown upon the canvas in strong colors :—"The true past departs not; nothing that was worthy in the past departs; no truth or goodness realized by man ever dies, or can die; but is all still here, and, recognized or not, lives through endless changes. . . . . It is but the superficial, as it were the body only, that grows obsolete and dies; under the mortal body lies a soul that is immortal, that anew incarnates itself in fairer revelation, and the present is the living sum total of the whole past." I believe that is true, although perhaps I could not prove it as conclusively to a questioning mind as in physics the corresponding law of the conservation of energy is proven. But as to continuity of type in the races of men, history and observation establish it beyond a doubt. The great facts of human nature are unchanged from one century to another except as they may be slowly modified by civilization, or religion, or other potential influences working upon the mind and heart of man, while in some aspects the springs of human action are as immutable as the laws of being itself. Hence it appears possible to ascertain the laws of social phenomena by experiments extending over long periods of time, whose growth is part of the organic life, where every condition is observed; and this is what is claimed for the historic method. Whether it be possible or profitable to make artificial experiments, I know not. Life is too short for economic tests ever to become popular, and practical statesmanship abhors them.

In other departments of human interest we learn wisdom from the experience of the past. The study of jurisprudence has made its greatest advance through the use of the historic method, as illustrated by Gibbon's famous chapter on Roman Law, by the earlier work of Montesquieu on the Spirit of Laws, and by the later work of Sir Henry Maine on Ancient Law. In legislation also the help of history is invaluable as a record of experiments in the making and working of laws. What indeed is the office of Parliaments and Congresses but the amending of old laws which are no longer suited to new conditions, or are shown to have been hurtful or inefficient in their operation, or never to have served the purpose for which they were intended—with at long intervals

the making of new laws to meet the growing requirements of the people? Not the least valuable of lessons is to be sought in the story of statutes that failed. We have seen frequent attempts made in the United States and Canada to restrict and control trade, commerce and labor by statute—even to regulation of the departmental stores. England made trial of all these restrictions centuries ago, but only to discover that they were ineffectual and worthless, as well as illiberal and mischievous. To such legislalators Albert Stickney's book on State Control of Trade and Commerce would be a revelation.

Let us not forget that society, or the State, is a developed organism, wherein every human will and passion has play, and every constituent individual acts and reacts upon every other, and that to understand it aright we must know its life history. The story of nations abounds with events of every kind, transpiring under an infinite variety of conditions, each event the product of some antecedent thought or act, and each making its impress for good or ill upon the life of the community, of the nation, or of the world, according to its own intensity or impact. To study such events and their operation and influence, in so far as they relate to inquiries into the provision of subsistence for the people and the supply of revenue for the State, according to Adam Smith—or into the nature of wealth and the laws of its production and distribution, according to Mill—or into the way the wants of the people in food, clothing, shelter, fuel, etc., may be satisfied, and how the satisfaction of these wants influences the national life and how they are in turn influenced by it, according to Roscher,—this is the office of the political economist. It is not merely to discover facts, but to collate them and discover the method for ascertaining the laws of the facts. "The laws of the economic constitution and movement of society," Ingram says, "are ascertained by observation, whether directed to contemporary life or to the history of the past. The office of deduction is then to verify and control the inductions which have been arrived at, using for this purpose considerations founded on the qualities of human nature and the external conditions to which society is subjected." I believe that to be a fair and intelligible exposition of the true method in the study of economic questions —being neither pure induction on one side nor pure deduction on another, but by an art akin to that of the great painter who mixed

his colors, "with brains, sir," to assemble things known (whether data, experiences or premises) in so natural an order as to make things unknown to be known and understood of all men. For the true method of experience, to paraphrase Bacon, first lights the candle to show the way; commencing with experience duly ordered and digested, and from it deducing axioms, and from established axioms again new experiments; "even as it was not without order and method that the divine word operated on the created mass."

It would be interesting, and it might be instructive, to illustrate methods as shown by the works of some of the great masters in social and economic science; but to do so would be to transcend the limits of this address. Roscher's example merits high praise, not alone for his working plan and the structure of economic doctrines he has reared, but for his thoroughness, his judgment, and the unfailing liberality he has displayed towards other workers. He does well, it seems to me, to draw his illustrations from the records of the more civilized nations for purposes of comparison, instead of casting a drag-net into the sea to gather of every kind. The very fewness of nations with an economic history makes it the more imperative to compare them all, as Roscher himself says. "Doubtless," he remarks, "comparison cannot supply the place of observation; but observation may be thus rendered more thorough, many-sided and richer in the number of its points of view." It will be noticed, however, that in his Principles of Political Economy Roscher employs the deductive method largely in the text, while the historical comparisons are grouped in footnotes, and are often so meagre in their setting as to be useless for any purpose of illustration without the help of a reference library to study the circumstances and conditions of every case.

I prefer the plan of Herbert Spencer in his Principles of Sociology, where the greater prominence is given to illustrations. Perhaps it was inevitable, from the character of the work, that these should be drawn from the accounts of barbarous peoples. But if a "fact" is not a fact it is unsafe for any use in comparison, and much of the information we possess of the lower races comes from sources that are not always trustworthy. A mere traveller is not a safe guide, for often he tells what he has heard and not what he has seen. "Trust no man as you can throw him; examine all you

can by your own senses," was the sound advice of Samuel Johnson on the study of natural history to a young physician, who was leaving England for Guadaloupe. Only the trained mind can do that, and any record of facts and observations concerning the lower races that is not based on the evidence of the senses must be taken with grains of allowance. No one will question the care shown by Spencer in gathering and collating his facts; but one may be pardoned for suspecting the utility of them as evidence, when he finds that after nearly fifty years spent in study and contemplation of the ultimate man in the light of them Spencer ended practically where he began.

As one who at the best is only an amateur economist, and disclaims any professional rank — yet as one who has given his best years to a study of the industrial life of not the least among the commonwealths of America — I humbly venture to show a better way, or what in my judgment is a better way. The same idea may have presented itself to others much abler and better qualified than I can pretend to be, and it may have been rejected as insufficient or unscientific. I do not know; I have not read all the literature of social and economic science. It often happens that theories and methods, even when original, do not need to be tried to prove what they are worth; they are condemned on the sufficient testimony of principles the soundness of which has stood the ordeal of centuries. But one of the great uses of history is to teach us what measures have been brought to the test of experience, and under what circumstances and conditions, and what the results of the test have been. My way, then, in brief, is to investigate in the light of human history the individual concrete cases which have exerted or were aimed to exert an influence on social and economic life, and from their operation and results to deduce "the laws of the economic constitution and movement of society." In using experiences to prove an abstract idea we cannot enter into all the circumstances of each experience without making the study interminable; but, taking each important concrete case by itself, with the human environment that influences it and modifies it, we shall find one of the best conceivable illustrations of the definition of history, that it is philosophy teaching by example. Like Roscher, I would confine investigation to events in the story of civilized peoples, from whom alone we can hope to procure trustworthy facts of economic value; and, like Spencer, I would seek to derive the

general principles of the science of economics from a presentation of social and economic data, and an accurate coördination of them. A few illustrations may serve to show more clearly the method I favor, but of necessity they must be brief, to the verge of baldness.

The land of Egypt affords one of the earliest instances of an economic experiment to be found in human annals, one which in modern phrase may be described as a scheme of national food reserve. There was probably never a time since its occupation by man when Egypt was not dependent upon the yearly Nile flood for the growing of crops to feed its inhabitants. It is the black river mud which renews each successive season the fertility of the land; and if the waters rise insufficiently by only one-tenth part in any year, so that the Nile does not overflow its channel back to the limestone hills which rise like walls on either side of the long level valley, the result is failure of the crops and famine. The story of the seven plenteous years and the seven lean years in the days of the Middle Empire, when the last Hyksos King was on the throne, is familiar to every Bible reader. There were only two men in Egypt who knew or believed what was coming to pass — the King himself and the young Syrian slave who understood the King's dream to interpret it; and the measures they took to provide against the years of famine were, as we know, wisely designed. The people themselves might have stored up grain and hoarded up money in the years of affluence and roaring trade, when the earth brought forth by handfuls; but the people never do. Their savings barely sufficed to carry them through the first year of famine, and that was a good showing. I doubt if there is more than one nation in our day that could make a better, if the grim necessity arose. When their money failed, when the last of it had been gathered up and brought into Pharaoh's house for the corn which they bought, and the wolf was at the door, the people went to the King to ask for bread. Why should they die in his presence when corn was laid up in the cities as the sand of the sea? But the King sent them away to his prudent counsellor and man of affairs, without whose leave no man might lift up his hand or his foot in all the land of Egypt — the ruler who bound the King's Princes at his pleasure and taught his Senators wisdom. "Give us bread!" the starving people cried, "for our money faileth." "Give your cattle," the ennobled young Syrian replied, "and I will give you

for your cattle, if money fail." And they brought their cattle, and Joseph gave them bread in exchange for horses and flocks and herds, and he fed them with bread in exchange for all their cattle that year. When that year was ended they came to him again. There were gaunt forms at home, wives and mothers in whose patient hearts love was dying, and hollow-eyed little ones sobbing and moaning for a crust, and the barns of the King were bursting with corn. "We will not hide from my lord," they said to this prototype of Shylock, "how that our money is all spent and the herds of cattle are my lord's; there is naught left in the sight of my lord but our bodies and our lands; wherefore should we die before thine eyes, both we and our land? Buy us and our land for bread, and we and our land will be servants unto Pharaoh; and give us seed, that we may live and not die, and that the land be not desolate." It is the old, old story; all that a man hath will he give for his life. Joseph bought the people and their land for the King, and he made it a statute that at the ingatherings they should pay a fifth unto the King — all of them, saving the priests, into whose order the King had married him! It makes the gorge rise to have a policy like that spoken of as statesmanship under divine guidance. It was not statesmanship. It was not even clever politics. It is a libel to say that God was in it. Little wonder that the King lost his crown, and that the kinsmen of Joseph were penalized under the new régime so cruelly that in the anguish of their souls the desire for liberty was lost.

My second illustration will be drawn from the reforms of the Gracchi, in the days of the Roman Republic. The old order of patricians, who were aristocrats by birth and virtue, had passed away, and in their stead had grown up a body of plutocrats, enriched by the spoils of the provinces, by trade, and by loaning money at high rates of interest. Their voice controlled the Senate, by which the laws of the State were administered, and they imposed numerous hardships on the free farmers of Italy. Wheat grown by slave labor in Sicily and other countries beyond the sea on the estates of the plutocrats was sold in Rome at a nominal price. Slaves, too, were employed on Italian estates in preference to free laborers, because they could not be called away to military service. The same land which, under the agrarian law, had supported one hundred to one hundred and fifty families of free peasants was occupied by perhaps fifty slaves, for whom, under the

Roman law, there was no marriage and no posterity. This was the situation in the republic early in the seventh century of the city, when Tiberius Gracchus appeared upon the public stage as a reformer. He was a young man of enterprise and ability. His father, though of plebeian lineage, was a distinguished soldier, and his ancestors for several generations had held the highest offices in the republic. His mother was a daughter of the most illustrious of the Scipios. When old enough to become a soldier, Tiberius Gracchus went into the army and fought at Carthage and in Spain. On the journey home his road lay through Tuscany, where he saw the large estate system in full operation — free citizens thrust away into the towns, and the fields cultivated by slave gangs — and he resolved to rescue the people's patrimony. His plan was to restore the old agrarian law, under which no man might occupy more than five hundred jugera of public land. The law had been originally passed to protect the race of yeomen, and Gracchus was determined to reëstablish them upon their farms. Returning to Rome, he began an agitation to this end, in which he had the support of a number of leading men of his own rank, who clearly saw the evil effects of a practice under which almost all the farm land in Italy was held by two thousand wealthy men. The common people, too, were quickly won over, for his cause was theirs. In spite of the Senate and the large land owners, Gracchus was elected a tribune by the vote of the comitia, and he carried his law. Its chief feature, after a certain reservation of rights to the occupiers of large estates of the public land, was the partition of the land in lots of thirty jugera (about eighteen and one-half acres of our measure) among the yeomen and burgesses, as heritable leaseholders, which they could not alienate, at a moderate rent to be paid to the State treasury. Under the old agrarian law, according to Niebuhr, the occupant was only a tenant at will, whom the owner (the State) had the right to turn out whenever it was thought necessary to apply the land to other purposes; and when a person possessed more than the law allowed he was liable to punishment, and the surplus was to be confiscated. But under the new law, the landlords might retain five hundred jugera, and as much more for their sons, and had besides a right to compensation for improvements on the lands taken away from them. Gracchus was a man of liberal views, but he was not an artful politician like the old rulers, who stood ready to take advantage of any mistake he might commit, and to lay plans

for his overthrow. The first commission chosen to carry out the provisions of the land act was composed of himself, his brother Caius, then twenty years of age, and his father-in-law, Appius Claudius. The nobles and the members of the plutocrat class objected to the personnel of the commission as having the flavor of a family compact, and even some of the close friends of Gracchus withdrew their support as a protest against his tactical blunder. A colleague of the tribunate was used by the Senate to thwart his purposes, and when the tribes met to choose tribunes for the ensuing year the Senators led an election riot, in which the reformer was slain and his body thrown into the Tiber. Caius Gracchus took up the mantle of his fallen brother, and showed that he was a greater master in the art of politics. "He was honest and pure like Tiberius," Niebuhr says, "but passionate, superior to him in power, and more definitely conscious of what he wanted." After a lapse of ten years he also was elected a tribune of the people, and one of his first steps was to avenge the murder of his brother and his friends. Many laws were enacted in the course of two years, but the one of greatest economic interest is the measure which ordered that corn should be divided among the burgesses of the city — to every one who should personally present himself — at a rate variously stated to be from one-half to one-fourth of the market price; and great warehouses were built for storing the corn, the ruins of which were to be seen at Rome down to the sixteenth century. The policy itself is open to criticism. "In order to understand this law," Niebuhr says, "we must remember that Rome received immense supplies of corn, and that its treasury was so filled that it was unnecessary to change the corn into money. At the time of the Social War the treasury contained about sixty millions sterling. The best use that could be made of it was to benefit the people, that is, the sovereign, and a vast number of whom were as poor as the poor in our own days. What should such a population of free men do? Were they to beg, or should the State support them? The idea of the dignity of every individual belonging to a free State lies at the bottom of many things which occur in a republic; it is the duty of a republic to take care of its members, even of the most insignificant, and this is to a certain degree the case with the poor rates in England." From another point of view, Mommsen says of the whole legislation of Gracchus that it was pervaded in the most

pernicious way by conflicting aims. On the one hand it sought the public good, and on the other it ministered to the personal objects and the personal vengeance of the ruler. "Gracchus," this historian says, "earnestly labored to find a remedy for social evils and to check the spread of pauperism; yet he, at the same time, intentionally reared up a street proletariat of the worst kind in the capital by his distributions of corn, which were designed to be, and became, a premium to all the lazy and hungry civic rabble." The distribution of the public or domain land under the agrarian law of Tiberius Gracchus had been well-nigh completed, but Caius resolved to extend its operations outside of Italy and to plant colonies in the transmarine territories of Rome. Various other measures were proposed, the object of which was to strengthen himself and overthrow the power of the nobles, and it seemed as if he had escaped the rock on which his brother had been wrecked. Whatever the purpose may have been, the effect of the corn laws was to gather into the city a mob of unemployed voters, living on State charity, clamoring at elections and crowding the circus, yet unwilling to be drafted off into settlements in Spain or Africa, where there would be work, instead of pleasant idleness. But when Caius Gracchus proposed to grant the franchise to the Latins outside of Rome, opposition arose. Those who regarded citizenship as a partnership were in no humor to enlarge the number of the partners, and the Senate at once began to play on this weakness. By another tribune it was proposed to release those who had received public land under the laws from the rent imposed on them, to declare their allotments to be free and alienable property, and, instead of looking abroad, to offer to the proletariat twelve colonies in Italy, each of which was to consist of 3,000 citizens, for the planting of which the burgesses might nominate suitable men. In this way Gracchus was beaten with his own weapons, and within less than a year he had shared the fate of his brother. But the fruits of his policy survived. The proletariat could not be denied cheap food as long as it possessed a voice in the *comitia*, and for five hundred years, Mommsen says, it lay like an incubus upon the Roman commonwealth, and perished only along with it.

From Joseph to the Gracchi is 1,600 years. I come down 2,000 years further for a third illustration, viz., the repeal of the corn laws in Britain, which, like the other two policies, was a food question to a large degree at its inception. The repeal of the corn laws was

not a freak of the English people. They had been debating the subject a long time, but there is no doubt that the failure of crops, the commercial distress and the high price of breadstuffs forced the hand of Parliament. The efforts to regulate the price of wheat by means of an import duty had succeeded only too well to satisfy the consumers, who were rapidly increasing in number with the development of manufactures in the early decades of the century. During the period of a commercial panic in 1825–26 authority was given to the government to suspend the duties and allow wheat to be brought into the country free, and the adoption of a more liberal policy by Parliament was only arrested in the following year by the death of Canning. In 1828, under the premiership of Wellington, a new sliding scale of duties, based on the price of wheat, was introduced. It provided for a rate of 25s. 8d. per quarter when the average price was 62s., gradually reduced to 1s. when the average price rose to 73s. and over. Other questions, such as Catholic emancipation and parliamentary reform, overshadowed the corn laws for three or four years. But failure of the harvest of 1836, together with the accompanying depression in business, renewed the agitation for repeal of the duties, the Anti-Corn Law League was formed, the people were aroused everywhere, and the weight of their influence became felt in Parliament. Another sliding scale was adopted by Peel in 1842, under which the duty was fixed at 20s. per quarter when the price was 50s. or under, at 12s. when the price was 60s., and 1s. as before when it rose to 73s. and over. But the hard times continued to help the cause of the advocates of free corn, and four years later Peel yielded to the pressure of public opinion. His measure provided that until 1st February, 1849, the duty on wheat should be 10s. per quarter when the market price was under 48s. and that it should fall a shilling with every shilling in rise of price to 53s. and over, when it was to remain at 4s. In 1849 the duty was to cease altogether, saving a registration duty of 1s. per quarter, which was abolished in 1869. But largely owing to the influence of Adam Smith and other economic writers a public opinion had been growing in England against all parliamentary contrivances for fixing prices and hindering the free industrial action of individuals. It was felt that men could manage their business affairs to the best advantage when they were left in such matters unguided and unrestrained by laws, and on every side the doctrine of laissez-faire was growing in favor. As a result, the

system of protection to industries by means of a customs tariff began to lose prestige as an occult agency, and other duties besides those on corn were attacked by the men of the new school. As early as 1823–27 Huskisson made a number of important reductions on the duties upon raw materials, besides freeing English and foreign shipping and restoring to artisans the right of free emigration. In 1842 many other customs duties were abolished or reduced by Peel, and still more in 1846; but reductions and remissions continued to be made at intervals, mainly under the direction of Mr. Gladstone, until the last vestige of protection disappeared from the British tariff in 1874. But the laissez-faire idea has not been permitted to triumph, although protection has been overthrown. During the last fifty years in Great Britain, as well as elsewhere, it has come to be understood that State action may conduce in an eminent degree to the prosperity of nations. Factory acts, sanitary laws, elementary and technical schools, agricultural experiment stations, railway aid, postoffice and telegraph service, municipal reform and many other agencies authorized by legislatures, and carried into operation by governments, have been instrumental in working out social revolutions of a highly beneficent character, not second in their utility to the material advancement that has left its impress upon nations during the last fifty years. Great Britain has truly made a marvellous record during this century; and in viewing it we are apt perhaps to exaggerate the influence of her free trade policy. When we see that her total foreign trade in the first forty-two years increased only from $375,000,000 to $625,000,000, that it immediately began to leap upward with the removal of restrictions in 1842, reaching $1,340,000,000 in 1854, $3,410,000,000 in 1873, and $3,726,000,000 in 1897—being an expansion of $3,101,000,000 under fifty-five years of free trade, in contrast with $250,000,000 under forty-two years of protection —there is a natural disposition to extol the potentialities of free trade. Especially is this the case when comparison is made with the growth of commerce under different trade conditions during the same period of time. But it is unquestioned that other and powerful agencies have been operating. Science and invention have rendered aids the extent of which no man can calculate. The steamship and the railway, without whose service modern commerce would be unattainable, are the creations of this century; and these in turn owe their greatness to development of the iron trade, whose

stupendous march in the last half century has no parallel as an industrial achievement in the life of man upon the earth. The policy of an open door at home, which Great Britain has pursued ever since the repeal of the corn laws in her dealings with other nations, has no doubt contributed sensibly to her commercial greatness; but perhaps no economist of any school is so confident of either his method or his abilities as to presume to assign definitely what share policy or science has had in the attaining of that greatness. If an engineer were asked to say which is the more useful and serviceable part of a railway, the locomotive or the rails, he would probably answer that both are essential parts of one system, that each is the complement of the other, and that either one if operated alone could achieve only an insignificant part of the efficiency possible when coöperating with the other.

Neither climate nor time seems to change the nature of man. The environment which influences him in a particular way in one age is likely to affect him the same way in another age. It is impossible to conceive a situation for him in which two and two will make five. Therefore it appears to me that there is no plan so well suited to make us understand what individuals, but especially communities and nations, will do under given circumstances and conditions as to know what they have done already. Nations do not entrust their affairs to men without knowledge or experience. Precedents are studied in every matter that is ripe for action, and the Minister who refused to consider what had been done in the same or similar circumstances by Ministers in office before him would be regarded as a dangerous man. Of course it is not implied that the wise policy is always to follow precedents; these may be, and sometimes are, examples to be shunned, and have their use and value in that way. And if that is the best method for Governments, which are constantly dealing with live issues, is it not clear that it is also the best method for economists, who are investigating problems that Governments may sooner or later be required to take up and settle on practical lines?

In all quarters of the world events have transpired during the lifetime of this Association that are probably destined to influence society powerfully throughout the twentieth century. We have witnessed the unification of Italy and the unification of Germany; the breaking down of monarchy in France and the setting up of a republic in its stead; a great Civil War in the United States and

the liberating of more than 4,000,000 of human beings born and bred in slavery; the partition of Africa among the great powers of Europe, nearly completed, and the partition of Asia among the same powers, under way; the confederation of the Canadian Provinces accomplished thirty years ago, and confederation of the Australian colonies on the eve of fulfilment; and within the last fortnight the signing of the protocol of a treaty of peace that removes the last vestige of Spanish rule from the American continent, and that inaugurates a new departure in national policy for the United States, imposing upon her new and enlarged responsibilities, and bringing her into closer touch than ever before with the European family of nations. All these events have quickened the world's life, and when affairs are finally adjusted we shall move, I trust, on a measurably higher plane in the twentieth century than we have moved in the nineteenth. It is inevitable that there will be social and economic changes, and it ought to be the business of economists to shape them in the light of the ripest experience to secure permanency, progress and peace.

PAPERS READ.

[ABSTRACTS.]

AN AMERICAN COLLEGE OF FORESTRY. By BERNHARD E. FERNOW, Ithaca, N. Y.

WITH this semi-centennial celebration of the American Association for the Advancement of Science, Section I is entitled to combine a quarter-centennial celebration of its action at the Portland meeting in 1873, which has this year culminated in a result for which the Section, and the Association of which it is a part, may justly claim its share of praise and congratulation.

It is nothing less than the establishment of a professional school, where a science and art is to be taught, which although well developed in other countries, was in this country twenty-five years ago hardly known even by name.

The springtide of this year has ushered in the organization of the New York State College of Forestry, at Cornell University, and with it the first professional center of the science and art of forestry in this country, an awakening of spring in the forestry movement after a long winter of sleep or battling with snow and ice, with frigid indifference or culpable lethargy. This event marks the opening of a new era for, when even the wild woods are placed under the influence of civilization and subjected to rational management, then indeed the pioneering stage of the nation may be said to have passed and stable and permanent development really begun.

There is no aspersion on the efforts of earlier or later endeavors when I claim that the memorial to Congress which, at the instance of the late Dr. Franklin B. Hough, was offered by Section I at the Portland meeting, endorsed by the Association and forwarded through the hands of an active committee, marks the starting point of the concerted movement which had in view the introduction of forestry, the application of rational principles in the use of our forest resources.

That action led to the recognition of the existence of such a science and art on the part of the Federal Government by the establishment of an agency in the U. S. Department of Agriculture which was to serve mainly for purposes of popular education on the subject. This agency later known as the Division of Forestry became at once the center of the movement and nearly all other developments have had more or less direct or indirect relation to this one.

Hitherto, although some sporadic work on professional lines has been done, the various agencies concerned in advancing the subject have concentrated their efforts, as was necessary, to arousing public attention to the evils resulting from the absence of the art, and to elucidating in a general way its nature and methods, thus familiarizing the people with some of its teachings, which finally culminated in securing such legislation as would in time lead to its application. Forest-fire legislation, forestry commissions with or without administrative powers, mostly with only educational functions, and forest reservations on public lands have been the result.

The growth of the idea among the people also induced some of the agricultural colleges and other educational institutions to offer short courses on Forestry, mostly of a cursory or synoptical nature, which being intrusted to professors of botany or horticulture naturally concerned themselves either with botanical or dendrological questions or else with certain phases of silviculture. While in no way professional from the forester's point of view, they have had undoubtedly a value in educational directions, interesting students in the subject and giving them correct ideas regarding certain parts of the same.

The New York State College of Forestry, however, is the first institution in this country, where a professional study of the whole subject in all its phases may be pursued with opportunities almost equal to the best in European forestry schools, excepting only the object lessons in the woods, which for the present are still absent, but for which provision is made.

Thus while the art of forestry is not yet practised, a place for the study of the science which must underlie the application of the art is established, a gratifying result of a twenty-five years' educational campaign for the advancement of the science.

It may be of interest to record the more direct causes leading to the creation of this novel educational institution. A continued agitation in the state of New York with regard to the necessity of looking out for the future of the forest cover of the Adirondacks, as the principal watershed of the state led to legislation, formulated by the writer in 1885 which, among other features, including especially an effective forest-fire law, created a Forest Commission to take charge of the forest lands belonging to the state.

These lands then, some 700,000 acres, having lapsed to the state for nonpayment of taxes after the valuable pine and spruce timber had been culled by the lumbermen, were found in larger or smaller separate tracts scattered over the 10,000 or more square miles, which constitute the Adirondack mountain country.

It soon became evident that to exercise an effective influence upon the condition of the forest cover, the state would have to control more of this land and finally a plan was matured, under which the state was to acquire gradually, by purchase or exercise of eminent domain, if necessary, all, or most, of an area comprising 3,000,000 acres in a compact body as a forest preserve. Under this plan the Forest Preserve Board, instituted for the

purpose of acquiring the land, spent last year a round million dollars and this year it has a half million at its disposal.

The original law of 1885, contemplated a forest management of the state property, and hence had conferred on the Forest Commission the right to cut and to sell wood. But no professional foresters were employed and no technical forest management was attempted; the right to cut and to sell was exercised simply in selling stumpage of spruce to lumbermen. The public, laboring under the mistaken notion, that forest preservation required cessation of all cutting of trees and not trusting to the discretion of its officers, rightly or wrongly, attempted to remedy the absence of technical advice by inserting into the constitution of the state a clause, which prevented all cutting of wood on state lands,—a foolish provision from the forester's point of view, but perhaps from the standpoint of expediency at the time commendable.

It became apparent that it was necessary to educate the people to a true conception of a rational forest management; and that an object lesson or demonstration was needed in order to show that forest preservation did not mean "Woodman, spare that tree," but "Woodman, cut that tree judiciously"—in other words, that forest utilization and forest preservation by means of forest reproduction were not incompatible.

Imbued with this idea, the superintendent of forests, of the re-organized or consolidated Fisheries, Game and Forest Commission in his report for 1896-7 suggested the establishment of such a demonstration of technical forest management.

Governor Black, becoming interested in the proposition, conceived the idea that such an experiment requiring a long time of continuous unchanged policy had best be removed from the ever-varying influences of politics and should be entrusted to a strong and stable educational institution.

This thought suggested the desirability of going a step farther, namely, to provide at the same time for the education of professional foresters, the future managers of the state forest property, when with the accomplished demonstration and the existence of competent mechanical advice the constitutional bar to rational forestry might be removed.

Cornell University was selected to undertake the experiment; a bill was drafted, introduced in the Legislature early in February, was passed and received the Governor's signature in April and on the sixteenth of that month, by action of the Board of Trustees, the University accepted the responsibility and established the college by the election of the writer as its Director.

Although a state institution, and as such dependent upon appropriations provided by the Legislature, the state college, while having its own separate faculty organization, is organically connected with the University and has the benefit of the entire plant of the same with some one hundred and eighty professors and instructors, at least thirty of whom will be called upon to furnish instruction to students of the college in fundamental and supplementary sciences.

In thus connecting a forestry school with a fully equipped university, the most advanced German ideas in forestry education are realized. While in that country in which forestry is best developed, of the nine higher schools of forestry only three are connected with universities, it has become more and more apparent to the educators and even to the directors of these separate institutions, that the advantages derived from the broader education and fuller equipments at the Universities far outweigh the advantages of the separate academies which were mainly seen in their location near the woods and demonstration areas. It is now recognized that, with modern methods of communication, the woods can be more readily reached from a railroad center, that at any rate only a limited amount of practical instruction is possible at the school wherever located the practice coming preferably after a thorough theoretical instruction; and, finally, it has been realized that the isolation of the academy is a detriment both to professors and students by its mental isolation.

For the present there are three teachers of forestry at the new college provided for, namely, the director and two instructors, with the possibility of utilizing the manager of the demonstration or college forest to assist, although the distance of the latter would make such participation of the manager in educational lines possible only during summer courses and on excursions to the school forest.

This teaching force is equal to the lowest requirements and can be satisfactory only for a beginning, while the college is in its infancy. The best German institutions have four and five full professors and only the less favored are satisfied with two full professors of forestry without administrative functions. The University of Munich has six professors of whom two are, however, teachers of forest botany and soil physics which are not exactly forestry branches but form fundamentals to be taught in the departments of botany and geology.

A satisfactory organization which would do justice both to the teaching and the research work, is more needed in this country than abroad and would require three full professors with at least two instructors, or assistants corresponding to the three groups into which forestry subjects can be divided according to their basis, namely, those which are mainly dependent upon natural sciences, and are concerned with forest crop production, as silviculture, forest protection, timber physics; those which are mainly based upon mathematics and engineering and are concerned with the practical management of a forest property, as forest exploitation, forest mensuration and forest regulation; those which are mainly of a philosophical character, requiring a full appreciation of all the bearings of the subject under which head forest administration, valuation and statics, history, statistics and forestry politics may be grouped.

The assistants would find ample work in aiding the professors in the lecture room, on excursions and practical demonstrations and in carrying on special investigations.

In spite of the deficiency in the faculty, which moreover is due to the smallness of the appropriation, namely, $10,000 for the first year,

which is limited to the expenses of the director and an assistant professor who acts also as forest manager, the courses of studies announced have been, nevertheless, made full, comprising twelve courses; the expectation being that no students for the senior year will be ready before the deficiencies in the faculty can be supplied with increased appropriations.

The course of studies leading to the degree of Bachelor in the science of Forestry has been made to cover four years, corresponding in this particular to the most advanced ideas of German educators in forestry rather than to the general practice. In this course the first two years are entirely given up to studies of the fundamental sciences, while forestry subjects and supplemental studies are left for the last two years. This arrangement has the advantage that forestry students may lay their foundation at any other educational institution and also should any entering upon the course for some reason desire to abandon their purpose, they will not have wasted any time on the specialty but find themselves prepared for any other study involving natural sciences as a basis.

The required fundamental and supplemental courses comprise altogether round 1270 hours, to which 450 hours supplementary work are added as elective but desirable, while the forestry branches represent about 600 hours of which 180 are optional, making an average of seventeen hours a week of required and elective work. In the fundamental branches, botany, chemistry and geology with allied sciences require the most attention, while a considerable amount of time is given to mathematics, entomology, political economy and engineering, especially map making and road building. It is also proposed to add a course on pisciculture and venery, since the future forest managers, at least on the state property, will undoubtedly have charge of taking care of the fish and game, which generally or often form parts of a forest property.

While these full courses are designed to equip, as completely as may be, managers of large forest properties, on which wood cropping is to be carried on as a business, the college will also endeavor to satisfy other classes of students who desire only a more or less cursory knowledge of the subject or some of its branches, such as students of political economy or owners of timberlands, who wish to practise silviculture at least. Finally, the requirement of the law, which prescribes that the college shall impart the results of the management of the demonstration forest, will be construed to mean that the function of the college is to pull up the stumps of ignorance regarding forestry wherever they present themselves. The University extension spirit will prevail. Finally, it may also establish summer schools designed to educate foremen and underforesters in forestry principles, an educational move which is as much needed as that for the training of forest managers for whom the college is originally designed to provide.

The one drawback under which the college will labor at first and indeed for some time to come, is the absence of object lessons in the field, the demonstration of principles applied in practice. To be sure, a demon-

stration area of 30,000 acres has been set aside in the Adirondacks, which will in time supply the deficiency, sooner in some directions, later in others, since results in forest management are as slow to accomplish, as the slowness of the crop would lead us to expect.

However, since the conditions in the Adirondack region are special, not only because of topography and climate, but because of the few species of timber trees involved, this single demonstration area cannot suffice and in order to secure variety of conditions other areas will have to be obtained. This does not necessarily imply purchase of such areas by the State; it will be possible in the future to secure the coöperation of private timberland owners, who may permit the college to prepare working plans for their lands and may be willing to apply forestry principles in their management, first in that generous spirit by which all our educational movements are supported and finally because of the financial benefits they may derive from it.

There are now some such attempts at forest management to be found in various parts of the country, which may be visited by students in excursions during vacation time. Finally, a visit to the European forests under management will be found profitable to students at the end of their studies and with full theoretical knowledge to guide them, even a brief visit will be of much interest and educational value.

The demonstration in the Adirondacks, as far as forest regulation and administration are concerned, may be made to form a pattern for almost any other region; as far as silvicultural methods are concerned, its teachings will be applicable, not only to the entire Adirondacks, but to a large part of the New England forest area, in which spruce is or has been the principal timber tree.

HIGH WAGES IN MONEY OR WHAT MONEY WILL BUY THE CONSEQUENT OF A LOW COST OF PRODUCTION. By EDWARD ATKINSON, Boston, Mass.

MR. ATKINSON presented the facts justified by the experience of the last fifty years and more, that the constant tendency in this country, which has been substantially free from militarism and in which trade is absolutely free, has been to lower prices, a lessened margin of profit, higher wages, and a very great increase in the aggregate of capital. He traced out the effect of invention in lifting those already possessing skill and aptitude in the arts to higher and higher positions, while enabling those who had been common laborers to become skilled operatives in a very short time in the direction of machinery more or less automatic.

He further held that there had been a full and complete justification of the axiom of Frederic Bastiat: "In proportion to the increase of capital the absolute share (of a given product) falling to capital is augmented, but the relative share is diminished; on the other hand, the share falling to labor is increased both absolutely and relatively."

He then took up the theory of Malthus published just a century ago upon which Darwin and Wallace both stated that they had derived the theory of natural selection and survival of the fittest. He then showed that the tendency which Malthus held to be a rule that population increased in a geometrical ratio, while the means of subsistence increased only in an arithmetical ratio has been completely disproved by the progress of the century. Had this theory been true the retrogression of the century would have been marked by increasing prices, less abundance, larger profits and lessening wages.

He further held that the theory of Malthus had no standing and ventured to suggest that Malthus, Darwin, and Wallace had dealt with man simply as an animal dominated by the forces of nature. He pointed out that according to the Jewish myth of the creation and throughout the history of thought mankind had proved to be endowed with the power to direct the forces of nature and to become a factor in production wholly beyond and aside from the mere physical forces or properties of labor.

He further held that while the dogma of Malthus had been discarded, the theory of evolution presented by Darwin and Wallace must be considered incomplete, the survival of the fittest falling to the mentally strong rather than to the physically strong. Therefore, while evolution might be considered one of the most important steps in the development of science that had yet occurred, it needed the further treatment on the metaphysical side, to the end that the evolution of the mental capacity of mankind and of the power of man to direct the forces of nature to ever increasing material welfare might be fully presented as a department of science.

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LOCAL LIFE BY LOCAL TIME. By S. EDWARD WARREN, C.E., Newton, Mass.

THE substance of this paper directed attention to the desirability and practicability of such "life," the "local time" being, however, *expressed* in standard time. The bearings of average and extreme discrepancies between local and standard time on many of the doings of daily life were pointed out, the contrast between "fast" and "slow" standard time noted; also the conditions on opposite sides of the boundaries between "time belts;" and the suggestion was made that census statistics might some day show a noticeable difference between vital, industrial and social conditions, far east and those far west of standard meridians — all for the purpose of calling the attention of social investigators to what *may* be something worth taking into account.

A STUDY IN COMPETITION AND SUBURBAN PRICES. By S. EDWARD WARREN, C.E., Newton, Mass.

THIS paper proposed competition between localities rather than between dealers in the same locality, and pointed out ways in which by a policy of concentration or consolidation of local dealers various reductions in expenses could be made, whereby suburban prices might be made as favorable as those of the large commercial cities.

EXECUTIVE DISCRETION IN THE UNITED STATES. By CORA AGNES BENNESON, A.M., LL.B., Cambridge, Mass.

1. DISCRETIONARY power conferred upon the Executive by the Constitution.

The Constitution created for the Executive a sphere of authority within which he is independent. It discriminated between what he is merely empowered to do and what he is obliged to do.

The earlier writers denied to the Executive any other powers than those enumerated in the Constitution, yet, the executive domain has been much enlarged by acts of Congress, decisions of the courts and independent executive acts.

2. Enlargement of the executive domain.

(*a*) By acts of Congress.

In the United States the Executive cannot initiate law; his participation in direct legislation is limited to the veto. He can, however, recommend measures and both his powers and duties have been increased by Congress under the requirement "he shall take care that the laws be faithfully executed." For instance, the first Congress decided that the power of appointment implied the power of removal, from which Jackson developed the Spoils System; Congress also increased the President's patronage by vesting in him the appointment of many inferior officers; it created the executive departments and extended them to subjects not contemplated by the Constitution; it gave him power to call out the militia, to decide when the United States government is bound to interfere and which of two rival organizations constitutes the legal government of a state; finally, by authorizing Lincoln to suspend the privilege of the writ of *habeas corpus* it established its right to confer upon the President unlimited discretion.

(*b*) Enlargement of the executive domain by decisions of the courts.

The courts will not decide abstract questions concerning the limits of the executive authority, but in litigation they have recognized that the duty of the President is not limited to the enforcement of acts of Congress according to their express terms. For instance, without positive provision of law, he may seize timber cut by trespassers on United States lands; he

may protect with an armed guard a judge of the United States Courts in the discharge of his official duties and a Federal officer who takes life in such defence is not answerable in the state courts; by interpretation his power to pardon has been extended to general proclamations of amnesty.

(*c*) Enlargement of the executive domain by independent executive acts.

In the process of executing the laws, the President may interpret the Constitution and statutes. The executive interpretation will then remain the law of the land until revised by new laws or court decisions. Washington interpreted the President's right to receive ambassadors as implying the right to refuse to receive them, an act by which war may be declared; Jefferson affirmed that he had no constitutional authority for the purchase of the vast territory beyond the Mississippi; Polk began the Mexican War before Congress had declared it; the Monroe Doctrine was the dictum of a president: Lincoln wrote the Emancipation Proclamation without consulting even his Cabinet, justifying it as a war measure.

3. Influence of war upon the competence of the President.

A declaration of war brings into action at once the full discretionary power of the President. Lincoln's extraordinary measures in the early part of the Civil War did not define how far the President may proceed without the aid of statutes, because Congress subsequently legalized his acts, but they changed the political estimate of the competence of the American Executive. Before then the executive was called the weakest department of the government: now we hear the cry that it is becoming imperial.

4. Delegation of discretionary power.

The President cannot delegate his legal or political responsibility. He can confer discretionary power upon his executive agents, but their acts are in law his acts. The Cabinet was not created by the Constitution nor by law; it has no collective responsibility. The President cannot allege the advice of his ministers as an excuse for mistakes.

The heads of departments issue the most important regulations. Their ordinances have the force of law and no appeal lies from their decisions to the President, except in questions of jurisdiction. They chose the majority of the employés attached to their services and may also remove them.

During our early history very little supervision was exercised by the higher administrative officers over the lower, because the statutes minutely defined their duties. Central supervision has increased with the development of the nation.

The mistakes of subordinate officials may generally be corrected by appealing to their superiors; if no remedy is obtained application may be made to the courts of the District of Columbia, thence an appeal lies to the Supreme Court of the United States.

5. Distinction between ministerial and discretionary acts.

When discretion and judgment are to be exercised by an executive officer the courts will not interfere. Ministerial duties they will enforce by mandamus.

Illustrated in the Patent Office where the Commissioner of Patents is

vested with discretion to decide whether the claim of an applicant is patentable; no appeal lies from him to the Secretary of the Interior or to the courts.

Also illustrated by the recent case of Williams *versus* Mississippi, in which the Supreme Court of the United States sustained the right of the election commissioners under the new constitution of Mississippi to use their discretion in judging of the qualifications of electors.

6. Control of executive discretion; its relation to civil liberty.

The courts cannot control the President in the performance of his executive duties; nor will they review his acts when the attempt will bring them into direct conflict with him.

Jefferson ignored a *subpœna* to appear at the trial of Aaron Burr; under the instructions of Lincoln an officer in the Federal army disregarded a writ of *habeas corpus;* the United States Supreme Court held that it had no jurisdiction of a bill to enjoin President Johnson from carrying out the reconstruction acts in Mississippi alleged to be unconstitutional and that if the injunction were allowed and the President refused obedience, the court was without power to enforce it. Judicially, the President can be reached only by impeachment.

A vote of Congress disapproving the President's policy need not affect him; bills attempting to prescribe his conduct he can veto. The Senate, however, where its coördinate is required, may obstruct him. It may pass bills over his veto, may reject the treaties he has negotiated, may refuse its coöperation in sending diplomatic agents, may not confirm his most important nominations and may so vest the appointment of inferior officers as to deprive him of a large amount of patronage. Congress may withhold appropriations to carry on the executive work and may reduce the President's office of commander-in-chief to a mere title by giving him no army and navy to command. Of these possible legislative restrictions two only have interfered with executive discretion, namely, the control over appropriations and appointments.

Attaching "riders" to appropriation bills was a device of Congress to control the President through the money power.

The Senate Appropriations Committee recently attempted to prevent the President from appointing the Canadian Commission by rejecting the item of $50,000 to defray its expenses. Had this not been restored, a resource remained to the President in the fund placed to his discretionary credit "to meet unforeseen emergencies arising in the diplomatic and consular service and to extend the commercial and other interests of the United States."

Congress may reduce the salary of any official except the President and judges. The service has occasionally lost valuable officers by not paying them salaries sufficient to maintain the social positions required of them.

Congress controls the President's power of appointment through the "Courtesy of the Senate," which obliges him to choose nominees acceptable to the senators of his party from the state in which the offices are to be filled. The Senate have converted the right to confirm into a power to

dictate and have thus captured a large amount of patronage that belongs to the President.

The Senate tried to exact from the President a statement of his reasons for each removal and all papers relating thereto, but in this was successfully resisted by President Cleveland.

The Senate has never refused to confirm the President's selection of his Cabinet except in the case of President Johnson; it also prevented him from appointing judges to the Supreme Court by passing a statute providing that no vacancy should be filled until their number was reduced.

Since the Constitution forbids United States officers from being members of either House and the heads of departments cannot, therefore, defend the policy of the administration publicly in Congress, the direct communication between the executive and legislative branches of government is through Congressional committees. These exert a very potent control. Their exactions from the executive departments in the way of statistics, special information and documents originated the expression "tyranny of Congress." Bryce calls them "a second meeting of ministers before whom the departments tremble."

We speak of the "President's Policy," but he cannot oblige Congress to deliberate upon any of the measures he recommends. The policy of the country is really decided in the standing committees, their only rival being the legislature caucus, in which the members of a party come to an understanding upon the position to be taken on the floor of Congress.

What relation does this legislative and party control of executive discretion bear to civil liberty?

The people know little of committee meetings, the details of their proceedings are not published. Still less do they know of the legislative caucus, which is private. The President is the one they hold accountable. If he is hampered in the use of his discretion, what security is there that the laws will be faithfully executed? It is important to the nation that he preserve his independence; even war may serve a good purpose if it conduce to this end, by calling into action his extraordinary powers as military executive.

There is no danger that the President will become imperial unless the people coöperate to make him so; if they ever do that, it will mean that they desire a fundamental change in government.

THE SHORT DURATION OF SCHOOL ATTENDANCE: CAUSES AND REMEDIES.

By Mrs. DANIEL FOLKMAR, Milwaukee, Wis.

THE conclusion reached by Professor Folkmar in a statistical study of the "Duration of School Attendance in Chicago and Milwaukee"[1] is, that of the pupils enrolled in the public schools, one-third never go beyond the

[1] Proceedings of the Wisconsin Academy of Science, Arts and Letters for 1897, p. 267.

first grade, one-half drop out before reaching the second grade, three-fourths fail to reach the grammar grades, nine-tenths do not go beyond the seventh grade, and only three in a hundred get into the high school, only three in a thousand graduate from the high school, and that the average, total period of schooling is less than three grades.

What are the causes of this short duration of school attendance? How may we lengthen the period of school attendance? Under present conditions of attendance how may we better provide for the child while in school?

Among the causes for the short duration of school attendance are the present social and economic condition of the country, the improper distribution of school rooms and teaching force among the several grades of pupils, unpedagogical and unsociological courses of study, lack of popular demand for the higher education of the masses and poor, compulsory laws of education.

Among the remedies for the short duration of school attendance and the better caring for the child under present conditions are: (1) the extension of the parental function of the state to the physical care of children when necessary during the period of compulsory school attendance; (2) an increase in the number of buildings so that not more than thirty pupils shall be placed in a room; (3) an increase in the number and the quality of the teaching force for the primary grades; (4) a pruning of the courses of study of the non-essentials and a restoring of the essentials (the despised three R's) to the place of first importance; (5) an increase in the number of kindergartens; and (6) the passage of a compulsory education law that can be enforced. Such law should require attendance at school between the ages of six and twelve whenever schools are in session, attendance at school for sixteen consecutive weeks each year between the ages of twelve and fourteen, and attendance at school between the ages of twelve and sixteen whenever not employed. The enforcement of the law to be made possible by laws providing for the physical care of children during school age when necessary, by child-labor laws, and by the establishment of truancy and ungraded schools.

The child who merely complied with the conditions of this law would receive the instruction of all the primary and grammar grades instead of less than three grades as is at present the case.

[This paper is printed in full in Journal of Social Science, January, 1899.]

MARITIME COMMERCE: PAST, PRESENT, FUTURE. By ELMER L. CORTHELL, M.A., D.Sc., New York, N. Y.

IT is not the purpose of this paper to discuss those general commercial features, which from time to time are brought out by the expert statisticians, like Mr. Mulhall and Mr. Kiaer of Christiana. These features will be only briefly referred to as related subjects, and having to do more with

the economies of Maritime commerce, with which it is not proposed to deal in this paper. The questions relating to naval vessels will not be discussed, for the reason that the dimensions of these vessels are not dependent at all upon the requirements of commerce, except as to depth of entrance channels, but upon the changing conditions of naval warfare, such as the advancing state of projectiles, defensive armor, methods of attack and defence, coal capacities, coast defence, and rapid movements upon the seas, disclosed by the recurring wars, like the China-Japan and the Spanish-American conflicts.

The main features of this paper will relate to the changes that have taken place since this Association was founded, in the last half century, in the method of maritime transportation from sail to steam, wood to iron and steel, effective carrying power of the commercial navies of the world, dimensions of ocean vessels, and the facilities of the harbors and ports.

The main purpose of the paper will be to form as reliable an opinion as possible of future requirements as to dimensions of channels and depths in them, and in the ports of the world, in order that the Governments, general and municipal, may know what they have before them in the way of harbor improvements and may at the opening of the twentieth century make the necessary provision for them in respect to plans and expenditures.

In order to bring the conditions more forcibly to the mind the various results of the investigations will, as far as possible, be exhibited by diagrammatic curves, which will be projected forward twenty-five and fifty years to convey an approximate idea at least of future requirements.

The investigations which have covered in all about seven years, and to which especially the last year of travel in Europe has been given, have been largely of the character of original research, the writer not having been able to discover results of investigations by other investigators which might be used for his purpose.

In ascertaining the facts and conditions, and obtaining a consensus of opinion by those qualified to give one, over forty ports of Europe have been visited personally. A correspondence with the Engineers of ports, which the writer was unable to visit, has been carried on, and all available sources of information have been utilized.

The principal sources of information have been Lloyd's Register of British and Foreign Shipping; Répertoire Général of the Bureau Veritas; The Shipping World Annual, London, and many of its weekly publications; U. S. Coast and Geodetic Survey; Report of the Chief of Engineers, U. S. A.; Coaling, Docking and Repairing Facilities of the Ports of the world, by the Naval Intelligence Office, U. S. Navy; Hydrographic Charts on file in the U. S. Navy Department; Tonnage and Movement of the Merchant Marine of the principal commercial countries by the Bureau of Statistics, U. S. Treasury; Highways of Commerce, Vol. XII, U. S. Government Printing Office; Commerce avec les pays étrangers pendant l'année 1896; Statistique de la Belgique, 1897; Annual Statement of the

Navigation Shipping of the United Kingdom, 1896; Estadistica General del Comercio Esterior de España, 1895, Madrid, 1897; Report of Minister of Finance, Vienna, 1897; Special U. S. Consular Reports, 1896; Mulhall's National Progress in the Queen's Reign, 1897, and his Industries and Wealth of Nations, 1896; Reports of the Minister of Public Works and of Marine of Italy; certain valuable compilations by Mr. Guerard, Engineer-in-chief of the harbor of Marseilles, exhibiting the growth of commerce of the ports of France, of Europe and of the principal ports of the world; the statements in correspondence by the Engineers of the principal ports of the world and the views of naval architects, ship-owners and ship-builders.

This information is of the very latest dates as well as of former years for purposes of comparison.

The writer himself has recently spent a month in the Library of the Institution of Civil Engineers, London, and in that of the British Museum upon the Engineering and Commercial features of the subject, particularly found in Lloyd's Register of British and Foreign Shipping and in the Bureau Veritas "Répertoire Général" — collecting, analyzing, comparing by periods since 1848 the voluminous data found in these annual Reports.

Not satisfied with the existing facts and those of the previous decades, he undertook to obtain the views of the Engineers of the ports of the world and of the naval architects, ship-builders, steamship managers, and agents, as to the future dimensions of maritime vessels, and the dimensions of channels for their use. To obtain them he addressed a circular letter covering all the points, to fifty-five Engineers in as many ports and to sixty-five naval architects, ship-builders, managers and agents.

It will at once be obvious that a mass of information would be gained by such extensive investigations of which only the principal results could be given in the brief space allotted a paper read before this Section of the Association. It will also be manifestly impracticable to give detailed references to authorities further than has been given above.

When this Association was organized fifty years ago, the Maritime commerce of the world was almost altogether carried in sailing vessels built of wood.

There were, according to Lloyd's Register of British and Foreign Shipping, only one hundred and twenty-one steamers, in 1848, only thirty-six of these built of iron; of the ten thousand five hundred and seventy-nine sailing vessels described, only seventy-nine were built of iron.

The development prior to this date had not been rapid; methods were experimental and efforts were tentative.

The radical change from sail to steam and from wood to iron came slowly. Commerce was conservative.

The first attempt to propel vessels by steam power was made at Barcelona, Spain, in 1543, then in France in 1707; in England in 1736; in the United States at Philadelphia by John Fitch in 1787; then came Robert Fulton in 1807; then on the Clyde in 1812, when it may be said that steam navigation was fairly underway.

The first steamer to cross the Atlantic was built in the United States at Savannah, Georgia, in 1819, by Col. John Stevens, but steam was auxiliary only.

The first British Atlantic steamer was built at Quebec in 1831. It crossed in 1838, and was called the "Royal William." The first passenger steamer crossed that year.

The first Cunarder crossed in 1840; the first steamer of the Collins Line in 1848. The first vessel fitted with a screw propeller was the "Francis B. Ogden," built by Captain Ericsson in 1837.

The first screw steamer to cross the Atlantic was the "Great Britain" in 1845. She was also the first iron steamer to cross the ocean.

The fastest passage, Liverpool to New York, up to 1840, was made by the Cunarder "Europa" in eleven days, three hours.

In 1852 the time had been reduced by the Baltic of the Collins Line to nine days, thirteen hours.

The "Royal William" made the passage from Quebec to London in a little over forty days; her dimensions were: length 160 ft., breadth 44 ft., depth 17¼ ft., 363 tons register.

Compare this ship with the "Kaiser Wilhelm der Grosse": length 625 ft., breadth 66 ft., depth 43 ft., tonnage 14,000, displacement 20,000 tons, 28,000 H. P., speed twenty-five miles per hour. The diminutive size of the "Royal William" may be appreciated when compared with one of the powerful English *tug boats* of the present day, 212 ft., 30 ft., 15½ ft., 712 tons, 1000 H. P.

Compare it again with the "Oceanic," now building at Belfast: 704 ft., 68 ft., 50 ft., displacement 25,000 tons, tonnage over 17,000.

It is not pertinent to the purposes of this paper to trace in constructive or mechanical detail the development of the means of maritime transportation. All information of this kind is on record.

The growth of steamships is traced and illustrated in a most interesting manner by Mr. A. J. Maginnis (member of the Institution of Naval Architects of Great Britain) in his admirable work entitled "The Atlantic Ferry," published in 1892, by Whittaker & Co., London. The writer has confined his investigations to the more general features of maritime commerce and has thought it best to illustrate the results by Plates of curves and graphical diagrams, which permit of an approximate forecast of the future and also some idea of the requirements of commerce and commercial facilities.

The curves of plate I, and the diagrams of plate II, relate to *dimensions* and *facilities;* the curves of plate III, and the diagrams of plate IV, relate to *general* features of Commerce. All features are arranged so as to be compared easily by the eye and their co-relations appreciated.

In projecting these curves and diagrams forward to cover the next quarter and the next half century, the restrictions and limitations imposed on man by the conditions with which he has to deal must be considered.

The past fifty years have shown a continual increase of steam naviga-

tion on all lines, as much in the latter as in the early part of the half century. Increasing the facilities has brought a continual increase in commerce.

Increase in number, size, speed and economy of steamships has augmented the amount and decreased the relative value of the goods transported. This economy has made it possible to transport goods of very low value long distances, which was not possible in the earlier part of the period. This economy has also brought new countries into the commercial field which formerly could not be reached at all. The sources of supply have thus been brought near to the markets where the demand exists. This beneficial change has occurred on the land as well as on the sea, by Canals and by the development of Railroad transportation, particularly in our own country of immense distances and of districts far removed from the seaboard.

This has been brought about mainly by increase in hauling power and in the size of freight cars.

At the beginning of the period they had an average capacity of only about ten tons (equal to the present capacity of the European car), whereas now thirty tons is an average, and even fifty tons is not unusual.

At the time this Association was founded four articles of relatively low value comprised not much more than one-half of the weight of merchandise exchanged between nations; namely, coal, grain, timber and metals; that is, nine millions of these products to seven millions of more valuable merchandise; now, it is as one hundred and twenty millions to sixty-five millions.

No doubt the decrease in cost of transportation had much to do with the notable fall in prices in the last fifty years. The average price of all articles of consumption was twice as great in 1848 as in 1898.

The increase in effective Horse Power on the sea has more than kept pace with the remarkable increase shown on the diagram in all other features of steam navigation.

In 1848 the total indicated Horse Power was *two* million; now it is *sixty* million.

For reference, and in order to follow the curves and diagrams and judge of the correctness of their projection into the future, the following data are given in tabular form.

The writer assumes the responsibility of all extensions of figures and curves beyond the year 1898.

PLATE I.

CURVES OF SPECIAL FEATURES.

Curve No. I. Average *Length* of the twenty largest steamships.

1848	230 ft.
1873	390 "
1881	460 "

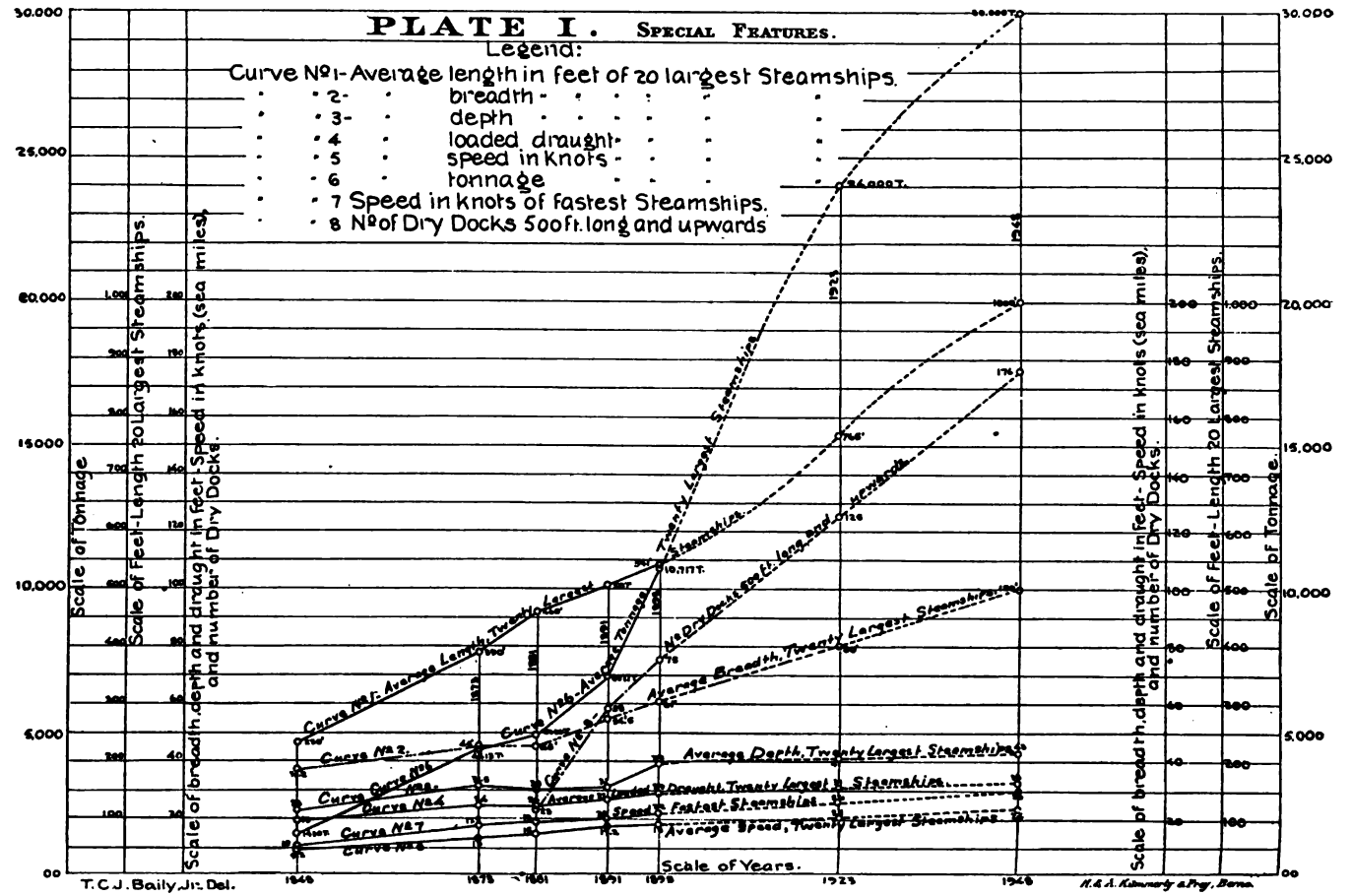


PLATE I. SPECIAL FEATURES.
Legend:
Curve No 1- Average length in feet of 20 largest Steamships.
" " 2- " breadth " " " "
" " 3- " depth " " " "
" " 4 " loaded draught " " "
" " 5 " speed in knots " " "
" " 6 " tonnage " " "
" " 7 Speed in knots of fastest Steamships.
" " 8 No of Dry Docks 500 ft. long and upwards
Scale of Tonnage
Scale of Feet-Length 20 Largest Steamships.
Scale of breadth, depth and draught in feet - Speed in knots (sea miles) and number of Dry Docks.
Scale of Years.
Curve No 1. Average Length Twenty Largest Steamships
Curve No 2.
Curve No 3.
Curve No 4.
Curve No 5.
Curve No 6.
Curve No 7
Curve No 8
Average Breadth, Twenty Largest Steamships
Average Depth, Twenty Largest Steamships
Average Draught, Twenty largest Steamships
Speed of Fastest Steamships
Average Speed, Twenty Largest Steamships
No Dry Docks 500 ft. long and upwards
Twenty Largest Steamships
T. C. J. Baily, Jr. Del.
H. & J. Kilvington & Prop. Berne

Curve No. 1 (*continued*).

1891	507	ft.
1898	541	"
1923	765	"
1948	1000	"

This is made up from the Répertoire Général of the Bureau Veritas for the years 1873 to 1898 inclusive; those for 1848 are estimated from Lloyd's Register of that year from the given tonnage.

Curve No. 2. Average *Breadth* of the twenty largest steamships.

1848	36.2	ft.
1873	45	"
1881	45	"
1891	54.5	"
1898	61	"
1923	80	"
1948	100	"

This is made up like No. 1.

Curve No. 3. Average *Depth* of the twenty largest steamships.

1848	23	ft.
1873	31.5	"
1881	30	"
1891	31	"
1898	39	"
1923	41	"
1948	43	"

This is made up like No. 1.

Curve No. 4. Average *Loaded Draught* of the twenty largest steamships.

1848	19	ft.
1873	24	"
1881	24	"
1891	27	"
1898	29	"
1923	31	"
1948	33	"

This, 48 to 98, is made up from data furnished on request of the writer, by the owners or builders, or from records in the ports where the vessels are, or were, owned.

Curve No. 5. Average *Speed* in knots (sea-mile) of the twenty largest steamships.

1848	9.2
1873	13

1881	15
1891	17.2
1898	18
1923	21
1948	24

This is made up like No. 4. All by recent correspondence for the special purpose of this paper.

Curve No. 6. Average *Tonnage* of the twenty largest steamships.

1848	1,430
1873	4,413
1881	4,900
1891	6,977
1898	10,717
1923	24,000
1948	30,000

Curve No 7. Speed in Knots, *fastest* steamships.

1848	10
1873	17.5
1881	19
1891	20
1898	22
1923	26
1948	30

Curve No. 8. Number of Dry Docks, 500 feet long and upwards.

1881	28 ft.
1891	58 "
1898	75 "
1923	125 "
1948	176 "

This is made up from the Annual Répertoire Général of the Bureau Veritas, and from information as to docks now building or planned, obtained by correspondence with the Engineers in charge.

PLATE II.

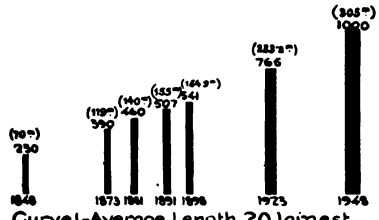
GRAPHICAL DIAGRAMS OF SPECIAL FEATURES.

Curves No. 1 to No. 5 are shown by *lines*.
Curves No. 6 and No. 7 are shown by *squares*.

PLATE II

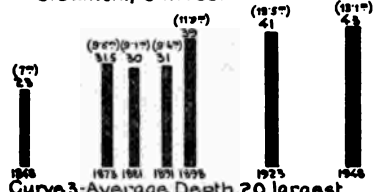
GRAPHICAL DIAGRAMS

To Illustrate Curves of Plate I.

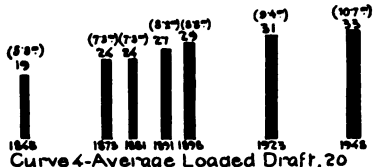


Curve 1-Average Length, 20 largest Steamships in Feet

Curve 2-Average Breadth, 20 largest Steamships in Feet.



Curve 3-Average Depth, 20 largest Steamships in Feet.

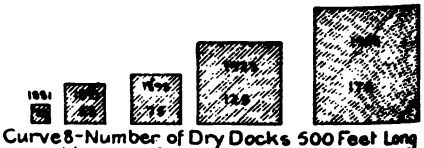


Curve 4-Average Loaded Draft, 20 Largest Steamships in Feet.

Curve 5-Average Speed, 20 Largest Steamships in Knots (sea miles).

Curve 6-Average Tonnage, 20 Largest Steamships.

Curve 7-Speed, Fastest Steamships in Knots (sea miles).



Curve 8-Number of Dry Docks 500 Feet Long and upwards.

T.C.J. Baily Jr. C.E., Del.

Lith. of Kennedy & Ray, Edin.

PLATE III.

CURVES OF GENERAL FEATURES.

Curve No. 1. Total *Number* of sailing vessels.

1873	56,281
1881	48,584
1891	33,879
1898	29,315
1923	17,770
1948	10,800

This to 1898 is made up from the Annual Répertoire Général of the Bureau Veritas.

Curve No. 2. *Total Tonnage* of sailing vessels.

1873	14,185,836
1881	13,872,881
1891	10,540,051
1898	8,894,732
1923	4,700,000
1948	3,241,000

This is made up like No. 1.

Curve No. 3. *Average Tonnage* of sailing vessels.

1873	252
1881	285
1891	311
1898	303
1923	265
1948	300

This is made up like No. 1.

Curve No. 4. *Total Number* of steamships.

1848	242
1873	5,148
1881	6,399
1891	9,638
1898	11,271
1923	14,925
1948	16,685

This is made up from Lloyd's Register of British and Foreign Shipping and from the Répertoire Général.

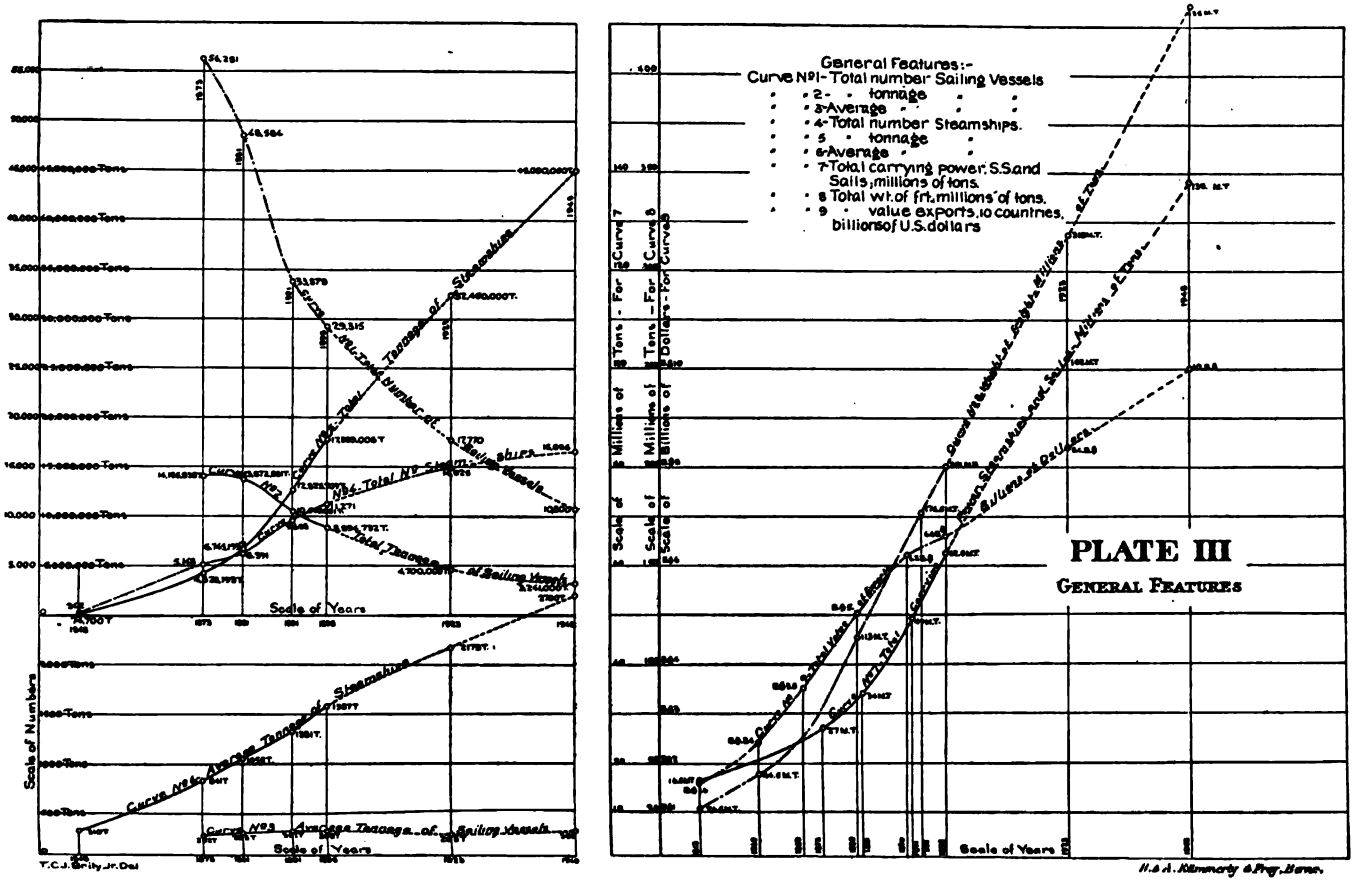

General Features:-
Curve No1- Total number Sailing Vessels
2- " tonnage " "
3-Average " " "
4-Total number Steamships.
5 " tonnage "
6-Average " "
7-Total carrying power, S.S. and Sails, millions of tons.
8 Total wt. of frt. millions of tons.
9 " value exports, 10 countries, billions of U.S. dollars
Scale of Years
Scale of Numbers
Curve No1 Total Tonnage of Sailing Vessels
Curve No2 Average Tonnage of Sailing Vessels
Curve No3 Average Tonnage of Steamships
Curve No4 Total Number of Steamships
Curve No5 Total Tonnage of Steamships
Curve No6 Total Number of Sailing Vessels
Curve No7 Total Tonnage of Sailing Vessels
Curve No8 Total Tonnage of Steamships
Curve No9 Total Value of Exports
Scale of Tons - For Curve 7
Scale of Millions of Tons - For Curve 8
Scale of Millions of Dollars - For Curve 9
Scale of Years
T.C.J. Smith Jr. Del
H.S.A. Kilmarnock & Eng. Boston.

PLATE III

GENERAL FEATURES

Curve No. 5. Total Tonnage of steamships.

1848	74,700
1873	4,328,193
1881	6,745,193
1891	12,825,709
1898	17,889,006
1923	32,450,000
1948	45,000,000

This is made up like No. 4.

Curve No. 6. Average Tonnage of steamships.

1848	310
1873	841
1881	1,055
1891	1,331
1898	1,587
1923	2,173
1948	2,700

This is made up like No. 4.

Curve No. 7. Total Effective Carrying Power of Steam and Sail, in millions of tons, on the basis of a ton of steam being equivalent to three tons of sail. From Mulhall's Statistics.

1848	16½
1873	27
1881	34
1891	49
1898	62½
1923	102
1948	138

Curve No. 8. Weight of Seaborne Commerce in millions of tons.

1848	26.5
1860	44.5
1880	113
1893	176.5
1898	201
1923	318
1948	435

This is from Mulhall's "Industries and Wealth of Nations," which covers the period from 1840 to 1893; the intermediate year of 1848 was obtained by interpolation, and 1898 by extending his figures by proportion.

Curve No. 9. Value of Combined Exports of the ten leading Powers of the Globe in billions of United States dollars.

1848	1.6
1860	2.4
1869	3.5
1880	5.0
1890	6.2
1896	6.5
1898	6.6
1923	8 4
1948	10.0

This is from the report of one of the leading statisticians of France, Mons. Jules Roche. His table covers the period from 1850 to 1896; the figures for 1848 and 1898 are obtained by using the ratios of his various years.

PLATE IV.

Curves No. 1 and No. 4 are illustrated graphically by *squares*.

Curves Nos. 2, 3, 5, 6, 7 and 8 are illustrated graphically by *cubes*.

Curve No. 9 is illustrated graphically by the *U. S. dollar mark*.

We have here seventeen curves, and sixteen diagrams which show the same features graphically.

Standing as we do at the fiftieth anniversary of this Association, midway of the first century of its existence, and sweeping the eye along these seventeen curves from the beginning in 1848 to the end in 1948, what do we see?

1. The average length of the twenty largest steamships is 230 ft. in 1848 and 1000 ft. in 1948.
2. The breadth increases from 36.2 ft. in 1848 to 100 ft. in 1948.
3. The depth increases from 23 ft. in 1848 to 43 ft. in 1948.
4. The loaded draft increases from 19 ft. in 1848 to 33 ft. in 1948.
5. The speed increases from 9.2 knots in 1848 to 24 knots in 1948.
6. The tonnage increases from 1430 in 1848 to 30,000 in 1948.
7. The number of graving docks 500 ft. long and over increases from 23 in 1881 to 176 in 1948.
8. The speed of the fastest steamships increases from 10 knots in 1848 to 30 knots in 1948.
9. The total number of sailing vessels decreases from 56,281 in 1873 to 10,800 in 1948.
10. The total tonnage of sail decreases in the same period from 14,185,836 to 3,241,000.
11. The average tonnage of sailing vessels in the same period remains practically stationary, being 252 in 1873 and 300 in 1948.
12. The total number of steamships increases from 242 in 1848 to 16,685 in 1948.
13. The total tonnage of steamships increases in the century period from 74,700 to 45,000,000.

PLATE IV

GRAPHICAL DIAGRAMS

To Illustrate Curves of Plate III

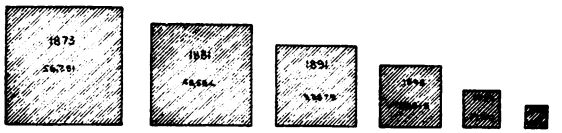


Diagram No 1 - Total No of Sailing Vessels.

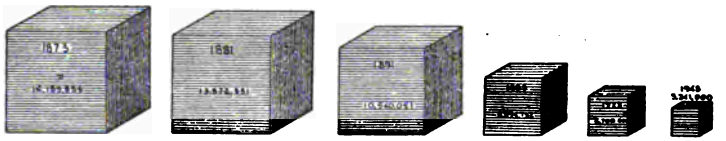


Diagram No 2 - Total Tonnage of Sailing Vessels.

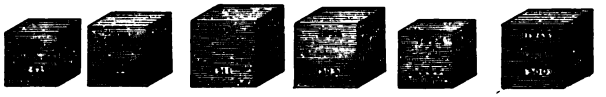


Diagram No 3 - Average Tonnage of Sailing Vessels.

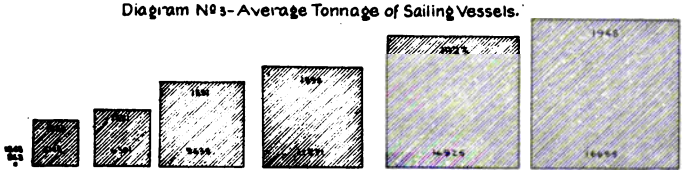


Diagram No 4 - Total No of Steamships.

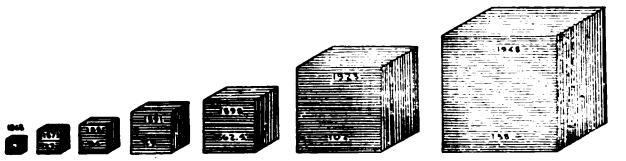


Diagram No 7 - Total Carrying Power, Steam and Sail, Millions of Tons

Diagram No 5 - Total Tonnage of Steamships

Diagram No 8 - Weight of Seaborne Tonnage. Millions of Tons

T.C.J. Baily, Jr. C.E. Del. Diagram No 6 - Average Tonnage of Steamships.

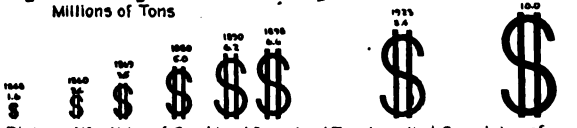


Diagram No 9 - Value of Combined Exports of Ten Leading Countries of The Globe in Billions of U.S. Dollars. E. & A. Ritmanly & Prop. Berne.

14. The average tonnage of steamships increases in this period from 310 to 2,700.
15. The total effective carrying power of steam and sail increases in this period from 16½ million tons to over 138 millions tons.
16. The weight of seaborne commerce increases in this period from 26½ million tons to 435 million tons.
17. The value of combined exports alone of only ten leading powers of the globe increases from $1\frac{6}{10}$ billions of dollars to 10 billions of dollars.

If the growth of tonnage in the principal ports of the world be compared with the general increase of Commerce shown on the diagrams, we shall have additional evidence of the remarkable increase brought about by steam transportation.

From reliable data furnished by the courtesy of Mr. Guerard, Chief Engineer of the Port of Marseilles, the conditions prior to the advent of steam and those now existing may be compared. Selecting twelve characteristic ports and taking the total tonnage of arrivals in 1832 (steam navigation did not begin until after that date) and the total in 1887 and 1895 we have the following:

1832	total	tonnage	of	arrivals	4,832,828.
1887	"	"	"	"	54,879,222.
1895	"	"	"	"	68,965,483.

The increase from 1832 to 1887 was over *twelve* fold and to 1895 *sixteen* fold, and that from 1887 to 1895, only eight years, was twenty-five per cent; the figures for 1896 and 1897, so far as obtained, show no diminution in the rate of increase.

The ports thus combined are

London,	Liverpool,	Marseilles,
New York,	Havre,	Hamburg,
Genoa,	Rotterdam,	Amsterdam,
Boston,	Antwerp,	Newcastle.

It is to be noted that at neither date is the coastwise, or domestic commerce of the United States ports taken into consideration, no complete record of it being kept; if obtainable it would considerably swell the percentage; for instance the foreign tonnage of New York harbor was less than 7,000,000 tons in 1896, whereas the total commerce of the harbor according to a U. S. Government Report was nearly 20,000,000 tons, and the total number of vessels passing through its channels, not counting tugs and ferry-boats, was 89,698.

The Domestic Commerce on the Atlantic and Gulf of Mexico coasts of the United States in 1893 was 72,701,973 tons. Very little of this appears in the ordinary commercial reports of entrances and clearances, which are almost entirely confined to foreign commerce.

Many special instances might be given as illustrations of the general growth of commerce.

In respect to the increasing size of vessels, at the Sunderland ship yards

they increased in twenty years only, 1863 to 1883, from an average of 410 tons to 1727 tons.

In respect to the over-sea tonnage at the port of London in the century from 1688 to 1792, the volume increased between six and seven fold; in less than a century from 1792 to 1882 it multiplied *forty* fold.

Plate V is a reproduction of a diagram made by Mr. Guerard and is here given for this purpose of illustrating the remarkable growth of commerce at the great ports of Europe.

The study of the tables and diagrams will make one important condition quite evident, viz.: that the draught of steamships is not keeping pace with their increase in length and breadth.

This may seem at first thought, a matter of comparatively slight importance, and the view may prevail, without careful consideration, that it is a question more of the theoretical than the practical.

Evidently the reason why the draught and, of course, the depth of steamships have been restricted, is that the vertical room for them cannot be as easily provided as the horizontal space; in other words, to provide the width of channels and entrances is not so great a tax upon the means at command as to obtain the depth through outlying bars, in canals, in river channels, into basins and docks and alongside of wharves and quays.

It may be stated, without danger of being controverted, that, if these depths are not increased, the size of steamships will not much further increase; and if it does not, the limit is in sight to the carrying power and the economies of commerce.

This important and possibly doubted statement requires some explanation. It must be conceded (the table and diagrams prove it at once) that the introduction of steam power upon the ocean has been one of the most potent and beneficent factors ever brought into the development of humanity. It made it easily possible to bring into the immediate influence of civilization countries that, prior to the advent of the steamship, were as far removed as though they were located on the planet Mars.

Now there is not a country in the world, lying any part of it upon the shores of the sea, that is not easily reached by commerce. Its products, whatever they are, beyond the demands of its own population, can and do reach quickly and economically the other nations of the world which require them for the sustenance and development of their peoples.

In the meantime steam upon the land has brought the products of the great interiors cheaply to the seaboard, where they are trans-shipped by maritime commerce, to other nations.

These changes have developed all countries to an astonishing extent, and the effect upon the transportation of the products of import and export and of domestic traffic has been tremendous.

The curves are all running *up hill* and will continue to do so unless checked by a false economy that may demand the withholding of the necessary means for deepening the commercial ways that connect the land with the sea.

Draught of water for the steamships of the present and future is the

desideratum to which urgent attention should be called by all those who desire the continued development of commerce and the still further cheapening of transportation and a greater reduction in prices.

I cannot state this important condition that confronts us any better than in the words of one of the leading naval architects of the world, Dr. Francis Elgar, the Consulting Naval Architect of the Great Fairfield Works on the Clyde, and the designer of the "Campania" and "Lucania." In a paper entitled "Fast Ocean Steamships," read before the Institution of Naval Architects in 1893, not long after these ships were built, he used these words (the italics are our own):

"*Deep Draught of Water.*"—This is a most important element of speed at sea, and it is now strictly limited by the depth of water in the ports and docks used by the fast passenger steamers on both sides of the Atlantic. Twenty-seven feet is the extreme limit of depth to which a ship can load on either side. "The "Campania" cannot load an inch deeper than the "Umbria" although she is 100 feet longer. If the underwater dimensions of "Campania" had been increased proportionately to those of "Umbria," her draught of water would have been 32½ ft. This class of steamer is increasing in length and breadth, *but the draught of water has to be kept the same.* The result is that it is only a question of time, and not of a very long time with our present materials of construction and type of propulsive machinery, *to find an absolute limit of speed imposed by the restriction of draught of water.*

. . . . . .

"It is not only that the present limited draught of water will finally impose an absolute limit of speed, other conditions remaining the same, but it has already a very prejudicial effect in keeping down speed at the point actually reached. If the draught were not restricted, the form of section could be improved by giving to it more *rise of bilge and an easier curvature.* . . .

"As the displacement is increased by increase of draught, the power required to drive a ton of displacement at a given speed becomes reduced. Hence increase of draught does not mean a proportionate increase of engine power, even when such increase is obtained merely by extra immersion without any improvement of form such as would otherwise be possible.

"The advantages of increased draught would be felt still more in a seaway than in smooth water, as the lower part of the hull would be less affected by the wave surface and better and more constant immersion could be given to the propellers. . . .

"The Atlantic trade is increasing at such a rapid rate that *larger* and *swifter* ships are certain to be soon called for. The depth of water has lately been somewhat increased at Liverpool; but much deeper harbours and docks will be required if further great increases of speed at sea are to be obtained without excessive difficulty and cost."

I hope Mr. George Holmes, Secretary of the Institution of Naval Archi-

tects of Great Britain, will not object to my quoting from his letter, dated May 18, 1898, as it bears importantly on this question: "As to the dimensions of ships to be expected in the future, this is the case of the ship-builder waiting on the Civil Engineer.

"It is tolerably certain that the lengths of steamers will not be greatly increased beyond the maximum lengths to which our largest mail steamers have attained until the Civil Engineers, by increasing the depth of water at the entrances of harbours and docks, enable the Naval Architect to increase the depth of vessels in proportion to their lengths. You may also be quite certain that the Naval Architect will take advantage of any opportunities which the Civil Engineer may give him."

The opinion of practical navigators and of commercial men handling large vessels is, that the inability to provide the depth of channels and harbors necessary to increase the draught of large freight carriers is very detrimental to good navigation and economical transportation.

As vessels increase in size they have to be built as flat as possible to allow them to enter the harbors, which injures their sea-going qualities and renders them more dangerous and unwieldy as well as more liable to injury from their shape.

Now, what is the Engineer doing to meet the desired increase in draught of steamships?

All that his employers have thus far felt justified in doing or expending. It is to them—the Governments of Maritime Nations, Port Trusts and Harbor Boards—that the facts and arguments for greater depths should be plainly and earnestly presented, notwithstanding the reluctance of the "powers that be" to improve the depths. It may be stated as a fact, palpable and undoubted, that no port of the world will, in the near future, be classed or used as a first-class port which will not readily admit steamers drawing at least thirty feet of water. This means thirty-five feet in the entrance channels through sea bars; thirty-two feet in river channels and other entrance approaches, and thirty-one feet in harbors, basins and along the quays and wharves. What is being done, if anything, to secure these depths?

First, and most important, the Suez Canal which was built with a depth of twenty-six feet is now nearly deepened to twenty-eight and one-half feet and the International Engineering Commission has made plans for the next deepening to nine metres, — practically thirty feet.

This canal is the key to all the harbors of Europe and Asia. What it has, all must have. The Amsterdam Canal opened with only 23 feet is nearly deepened to 30 feet. The new Kaiser Wilhelm Canal has nearly 30 feet. Liverpool has now an entrance channel of practically over 30 feet at low tide, — 27 to 57 feet. Southampton has 30 feet at low tide, New York will have, by the present plans, 35 feet at low tide, Boston will have 30 feet low tide, Philadelphia the same. New Orleans, in the plans for an enlarged channel-way, will propose to the United States Government a least depth on the sea-bar of 35 feet and at least 32 feet in the South West Pass and Mississippi River. Galveston, by the present plans now being

carried out, will have 30 feet. Hamburg will, in the near future, have a deep channel-way to the sea adequate for the largest vessels. Antwerp has a sea entrance 30 feet deep, a river depth of 31 feet at high water and a projected depth of port area of 32½ feet. Other ports might be mentioned where deeper channels are in progress of construction or are projected.

In order to appreciate the changes that are going on under an inexorable law of commerce, a few instances may be mentioned among many as illustrations.

In 1874, less than a quarter century ago, Mr. Jas. B. Eads, with that prescience so eminently characteristic of his genius, proposed to the United States Government to deepen the mouth of the South West Pass of the Mississippi River (where there was at the time only about 12 feet of water) to 30 feet, with 350 feet width at that depth, which would have given a navigable central depth of at least 35 feet.

The mouth of the South Pass, about ¼ the size of the South West Pass, was given him by Congress to deepen to 26 feet with a central depth of 30 feet; the twelve miles of this pass and the Head of the Passes were to have a Channel of a navigable depth of 26 feet, which was at that time considered sufficient for the navigation of the Port of New Orleans.

Now, it has become evident by the increasing size of steamships, that the little Pass is entirely inadequate for even the present requirements, much less for those of the near future. Vessels trade at New Orleans, that, fully loaded, carry 11,000 tons of dead weight on a draught of 29½ feet. They can now load at New Orleans to only two-thirds of their capacity. The decided opinion of commercial men at New Orleans is that the steamships of the future must have a channel thirty-five feet through the sea bar.

Their experience, as well as that of all commercial men, is that the larger the vessel, the greater the economy of transportation, and that vessels for the American trade at least have in recent years been built just as large as the United States ports and those in Europe can accommodate, and that, this being the case, the limit of draught and depth of channel has not yet been reached.

Vessels will continue to increase in size, especially since facilities for deepening harbor entrances have also greatly increased in power, economy and rapidity of execution.

Liverpool is being improved beyond all other ports of the world and is expending immense sums to provide for the steamships of the future. Weary with undoing the work of the past, tearing down or rebuilding what was completed, or rebuilt, only a few years previous, the Dock Board has now, with singular far-sightedness, planned really for the future; and it is widening and deepening the docks to accommodate vessels up to 900 feet in length and to 90 feet beam and 36 feet draught.

It is building a dry dock for vessels of this class, and is making plans for one 1000 feet long, expending at present $17,000,000 on enlargement of these facilities.

At London the vexatious bars and narrow tortuous channels near the mouth of the Thames, through which it is dangerous in certain stages of tide and in fog to navigate vessels drawing over 26 feet, are to be removed and improved and a navigable depth of 30 feet provided.

The limited space accorded a paper upon so large a subject, which has, for even general statements, required such a broad field of investigation, permits of only the most general discussion, and a selection of only a very few of the numerous facts collected by an extensive correspondence.

Enough, however, has been stated to show the vast importance of the subject of Maritime Commerce from an economic point of view and the desirability of meeting the requirements of this commerce, which has revolutionized the great economic conditions of the world.

CUBA: PAST, PRESENT, AND FUTURE. By Dr. WOLFRED NELSON, F.R.G.S., New York, N. Y.

CUBA, well and happily named the "Queen of the Antilles," is the largest island in the West Indies. The island is divided into six departments. Taking them in order from west to east, they are as follows: Pinar del Rio, or the western province; next the provinces of Havana, Matanzas, Santa Clara, Puerto Principe, and Santiago de Cuba, the latter also known as the eastern province. A line drawn from Cabo de San Antonio, or Cape San Antonio, the extreme westerly point of Cuba, extending centrally through the island to Cabo de Maisi or Cape Maisi, measures seven hundred and eighty-four English miles. The form of the island is somewhat irregular — a semi-crescent — for two-thirds of its extent, its convex side towards the Bahama Islands and the United States, its concavity towards Jamaica.

To make my brief paper on Cuba comprehensive, the *ante bellum* politics of Cuba must be referred to. There were four parties — the Conservatives, or Spanish party; the Liberals, equally loyal to Spain, but with a broader platform, and the Autonomistas who wanted Cuba to have the freedom under the Spanish flag that Canada enjoys under the British, and the Separatista, or revolutionary party, a mere handful relatively to the bulk of the population, who wanted Spain overthrown—death to Spaniards, in fact death to all who failed to agree with them. This very small but troublesome faction consists of blacks and their descendants, with a few equally irresponsible whites — men lacking in influence, having no real interests in Cuba. That party initiated the late revolution. It was maintained and engineered from without by Cubans who had become citizens of other countries, the better to enable them to hatch rebellion under neutral flags. They and some exotic *fin-de-siècle* buccaneers, as Señor Dupuy de Lome happily termed them, they and their backers are wholly responsible for the revolution. The rebellion, properly so called, had been crushed. It had become a movement of lawless blacks led largely by a few alien adventurers. Then came the latter phase when the United

States took an active part. We, one and all, know that the soldiers and sailors of Spain and the United States fought well and nobly. The gallant Cervera, acting under the orders of Blanco, sailed out of the harbor of Santiago de Cuba to almost certain death. His fleet was destroyed. He and his surviving officers and men were made prisoners of war. Cervera's prompt recognition of the daring and bravery of Hobson had endeared him to the American and to all other appreciative people. Spain's grand old man came here a captive merely to capture his captors. Cervera's name will go down in history as one of nature's noblemen.

Jeanne d'Arc said "*Pourtant la France ne meurt jamais*,"—France never dies. The same may be said of fair and sunny Spain. A country producing sailors and soldiers like hers is indestructible. Spain with her fertile soil and industrious people will maintain her place in the family of nations. She has lost her West Indian possessions, she has been defeated by a powerful and modern nation — five-fold stronger than she is. She has met with honorable defeat. Her honor and her history have suffered naught. Her history has assumed a modern phase, showing the world at large that Spain and her people have lived up to their best and bravest traditions. Long live Spain! Long live the Queen Regent! Long live the King! We may thank God that peace has been concluded — a peace honorable alike to victor and vanquished.

What will be the fate of the Queen of the Antilles? The last census of Cuba was taken in 1887. Taking the provinces from west to east it was as follows: Pinar del Rio, white, 167,160, colored, 58,731; Havana, white, 344,417, colored, 107,511; Matanzas, white, 143,169, colored, 116,401; Santa Clara, white, 244,345, colored, 109,777; Puerto Principe, white, 54,322, colored, 13,557; Santiago de Cuba, white, 157,980, colored, 114,339; totals, white, 1,111,303, colored, 520,316. Total population, 1,631,619.

Let us develop our facts by making a few comparisons. The area of Cuba and adjacent islands is, say, 45,000 square miles. The area of the United States of America is 3.008,400, exclusive of Alaska; including Alaska, 3,585,790. By dividing the area of Cuba into that of the United States of America, we have, as a result of the calculation, 79 $\frac{39738}{45000}$. Cuba is, say, one-eightieth of the area of this great and grand republic.

In 1892 the total exports and imports of the United States of America were $1,857,680,610; excess of exports over imports, $202,875.686; the latter being the balance of trade in favor of the United States.

In 1892 the total exports and imports of Cuba were as follows:[1]—

Total imports under Spanish flag . . .	$45,109,992 00
Imports under other flags	24,334,095 00
	$69,444,087 00
Total exports under Spanish flag . . .	18,592,565 79
Under other flags	82,421,700 39
	$101,014,266 18

[1] El Boletin de la Camara Oficial de Comercio, Havana, March 31, 1893.

Imports	$69,444,287 00
Exports	101,014,266 18
Total	$170,458,553 18
Deduct 10 per cent to reduce to United States gold	17,045,855 31
	$153,412,697 87

Balance of trade in favor of Cuba, for 1892, reduced to United States gold, $31,254,276.40. By multiplying Cuba's balance of trade by 80, it gives $250,031,217.20. Had the United States done as well area for area as Cuba, the above would have resulted. Her balance of trade was $202,875,686.

In 1850 the trade of Cuba was $50,000,000 or $45,000,000 United States gold, while that of the United States for 1850 was $317,885,252.

Cuba's trade per inhabitant (population, 1.612,000, trade $153,412,697.87) is $90 per capita; Uruguay (population, 700,000, trade $60,390,010) $74.70 per capita; Costa Rica (population 243,000, trade $16,200,000) per capita $40.50. Chili (population 2,800,000, trade $127,000,000) per capita $38; Hayti (population 572,000, trade $18,000,000) per capita $31.50; United States of America (population (1892 estimated) 65,000,000. trade $1,857,-680,610) per capita, $28; Porto Rico (population 800,000, trade $29,000,700) per capita, $24.80; Venezuela (population 2,300,000, trade, $32,400,000) per capita, $17.05.

Cuban crops are: —

Sugar, 1878	530,596 tons
" 1888	630,311 tons
" 1894	1,018,028 tons
Tobacco, value, 1879, United States gold	$15,804,000
" 1894	17,746,100

Shipping in 1894: —

Arrivals, 3,748; tonnage, 4,388,555.

Cleared, 3,713; tonnage, 4,050,488.

Railways in Cuba, 1515 miles.

The tax on sugar in 1892 was 10c. a 100 kilograms (220 lbs.), 5c. a 100 kilograms on Muscovado and molasses, or 9c. and 4½c. United States gold. In 1893 the above taxes were reduced by one-half. In February, 1895, they were abolished.

Island expenditure 1895 and 1896: —

In Spanish gold		$26,095,244 19
Revenue: —		
Direct taxes and imports	7,049,500 00	
Indirect taxes, custom house revenue stamps, lottery, public lands and property, contingent, etc.	17,706,259 00	
		$24,755,759 00

While Cuba produces sugar and tobacco to the annual value of $100,000,000 gold, the taxes are calculated on the net profit of the crops. To calculate taxes, take any crop of say $1,000 estimated profit $400 the owner would pay taxes on $400 at $2 per $100 or $8.

In 1878, the ten years' war closed. It ended in the Zanzon treaty.

Estimated expenditure: —

1878–1879	$46,594,688
1885–1886	31,169,653
1890–1891	24,446,810
1893–1894	26,137,394

Comparative taxation: —

Chili	$23 per capita
Brazil	$22 " "
Uruguay	$20 " "
Costa Rica	$19 " "
Argentine	$16 " "
Hayti	$16 " "
Cuba	$15 " "
Porto Rico (no war debt)	$ 8 " "

The figures and comparisons I trust will be interesting to students of economics. Remember her small population, and that only about one-third of her area is cultivated. If necessary she can multiply her products five to ten fold. When the lawful land-owning interests in Cuba vote, no doubt it will be for an independent Cuba under an American protectorate. If the Marquis de Montoro, Señor Galvez and men of that highly educated influential type, men who are leaders of the autonomists—if they could put their advanced views in force, it would be an ideal arrangement and all would be well. The Marquis de Montoro wanted a Cuba under the Spanish flag as I have already stated. Their views did not anticipate the present state of affairs. Alas! being law-abiding, thoughtful men, they are denounced by the small lawless horde of blacks. Señor a Cuyas, recently designated that faction as scum. They constitute a mass of insular scum, whose natural instincts are incendiarism, thieving and murder, varied at times by the outraging of young and defenceless women. Such are the liberators of Cuba. The Cuba Libre of the blacks would be a veritable hell upon earth, a blot upon Christian civilization.

Cuba, the fair and fertile, to take her place in the family of nations, must have law, order and peace guaranteed by the United States. Cuba, to be a creditable part of the earth, must be ruled by a firm hand. A rebellious beggarly minority, largely the criminal elements in the island, must be taught that the great law-abiding majority have rights that must be respected. That 5,000 or 10,000 ragged negroes should dictate terms to over a million and a half of people is the acme of audacity, but their sole possessions are semi-nudity and audacity.

Knowing that island as I do, I fear that an independent Cuba will be an impossibility. As an American colony she will blossom and bring forth her increase. Then, and then only, will the black plague of central and east-

ern Cuba cease to be a nightmare. It is a question of time. Cuba will be the brightest spot in the colonial possessions of the United States. Old conditions have passed away. This great and glorious republic must face her destiny. Recall the words of an early day, Prince of Sweden, looking at a picture of some acquired territory, in a moment of inspiration he wrote under it—"God gave it to me, the devil shall not take it away."

To the rich, fair and beautiful Queen of the Antilles let us say—Adios Cuba Americana, Dios te guarde. Fare thee well, American Cuba, God protect thee.

EXAMINATION OF THE THEORY OF RENT. By EDWARD T. PETERS, Washington, D. C.

THE economic doctrine of rent is based on the assumed mobility of labor and capital and on the tendency of these factors of production to earn equal returns for equal expenditures of the one or the other.

When the period of diminishing returns is reached on land of any given quality, the next dose of capital and labor produces a smaller return in proportion to its amount than has been produced by the doses previously applied, and this diminished return becomes the measure of the returns per dose to all capital and labor, since there cannot theoretically be more than one rate of compensation to either of these factors at a given time.

It is true the doses of capital and labor previously applied to the land continue to produce a larger amount per dose than the increment obtained by applying the last dose; but, if capital and labor can be obtained at a cost equal to the value of this increment, all produce in excess of this value will accrue to the landowner as such—not as capitalist or laborer, supposing him to unite the functions of one or both of these with his function as landowner. And whatever income accrues to him simply in virtue of his ownership of land, over and above the return for any capital or labor expended in the cultivation of it, or the making of improvements on or in it, comes within the definition of the word rent as used in connection with land in political economy.

If a further application of capital and labor to the land results in a still lower return to the additional dose, this in its turn will become the measure of the returns to all the capital and labor employed, and the preceding doses will yield a rent equal to the excess of their respective products over the product due to the last dose. Thus if the first one hundred doses produced at the rate of half a bushel (0.50 bushel) of wheat per dose, the next five doses at the rate of only 0.45 bushel per dose and the next five at the rate of only 0.40 bushel per dose, 0.40 bushel per dose would measure the returns to all capital and labor, the doses from the 106th to the 110th inclusive producing no more than their own compensation, while those from the 101st to the 105th inclusive would, under the supposed

VARIATIONS IN THE RATE OF AGRICULTURAL PRODUCTION AND ONE OF THEIR CAUSES. By JOHN HYDE, Washington, D. C.

THE twenty years ending with 1897 witnessed the harvesting in the United States of crops of corn, oats, and rye the yield per acre of which was from 50 to 60 per cent greater than the corresponding yield in certain other years of the same period; of crops of potatoes in which it was from 80 to 87 per cent greater than in other years of the period under consideration, and of crops of buckwheat in which it was from 80 to 130 per cent greater than in the case of certain other crops of buckwheat grown within this same period of twenty years. On the other hand, the highest annual yields per acre of wheat, cotton, hay, barley, and tobacco were only 50, 39, 39, 36, and 23 per cent respectively higher than the lowest. This remarkable non-uniformity of fluctuation has suggested to the author of this paper the operation of some law not hitherto generally recognized, and the examination of the statistics of a large number of crops for each separate state during a period of twenty years shows that entirely independent of whether the average yield per acre be high or low, the nearer the approach to the region to which a product is indigenous the more uniform will be the rate of production from year to year, and the further the departure from such region, the greater the liability to fluctuation.

For the purpose of this abstract, four products only need be considered: oats, barley, cotton and corn. The period covered is twenty years, 1878–97, and the comparison is based in each case — not upon the two extreme deviations, but on the means of the three highest and the three lowest yields per acre in the twenty-year period, the figures given representing the per cent of the deviation of these means from the mean of the entire period.

In the case of oats, in twelve of the most northerly states of the Union (the Transition zone[1] of the Merriam Life Zone Map) the deviation from the twenty-year average was only 34.23 per cent, only two states exceeding 40 per cent; in the Upper Austral (from New Jersey, Delaware and Maryland to Kansas and Nebraska) the deviation was 53.95 per cent, only one state having less than 40 per cent, and in the Lower Austral (from Virginia, the Carolinas, and Georgia to Texas and Arkansas) it was 62.78 per cent, no state falling below 50 per cent. In the case of barley the deviation in the Transition zone was 37.7 per cent, in the Upper Austral 59.5 per cent, and in the Lower Austral 69.9 per cent.

On the other hand, in the case of corn and cotton, it is with the extension of their cultivation northward that the range of fluctuation in the average rate of production is found to increase. In the case of cotton, this variation was 25.1 per cent of the average yield per acre in Alabama, 26.3 per cent in Georgia, 35 per cent in Mississippi, 37.9 per cent in South Carolina, 40.4 per cent in Louisiana, 41.3 per cent in North Carolina, 42 per

[1] The transcontinental belt in which Boreal and Austral elements overlap.

cent in Arkansas, 53 per cent in Texas,[1] 54 per cent in Virginia, 55.5 per cent in Florida,[2] and 75.3 per cent in Tennessee.

Corn does not exhibit the same regularity of progression, owing (1) to the large acreage in the semi-arid portions of Texas, Kansas and Nebraska, where the frequent deficiency of moisture is a disturbing element; (2) to the extent to which special varieties have been adapted to local conditions to meet a want that no other crop can satisfactorily supply, and (3) to the extreme care with which this greatly esteemed product is cultivated in certain sections where its growth is precarious.[3] Still the variation in the Upper Austral zone, excluding Kansas and Nebraska,[4] is 49.69 per cent, against 38.46 per cent in the Lower Austral, exclusive of Texas; [4] and if, for the reasons above stated, that of the most northerly tier of states, excluding Maine and Rhode Island,[4] is only 44.57 per cent, it is a significant fact that there is not a state in this belt with as small a variation as Alabama or Florida, and that there is but one that will compare favorably with Georgia, Mississippi, Louisiana or Tennessee.

Investigations show that this law of diminishing constancy is entirely independent of whether the average yield per acre is high or low, and that there is no general correspondence between its operation and the annual variation in the rainfall. The non-uniformity in the fluctuations of various products is attributed by the author to the different proportions of such products grown at a greater or less distance from the natural habitat.

RAILWAY RATES AND COMPETITION. By H. T. NEWCOMB, Department of Agriculture, Washington, D. C.

THOUGH all economists and competent students of transportation problems have become convinced that competition among different railways, offering to perform identical services, has no salutary regulative effect upon the changes for railway services, there is still a belated public opinion favorable to such competition that has had abundant expression in state and national legislation. State constitutions, statutes and charters of particular railways have forbidden the consolidation of parallel lines and interdicted agreements for the division of competitive business. Later, Congress burdened the Interstate Commerce law with the incongruous

[1] The somewhat wide fluctuation in Texas is due to the extension of cotton planting into regions of uncertain rainfall.

[2] Not altogether reliable, owing to the non-determinable proportions of the upland and sea island varieties.

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fifth or anti-pooling section and, finally, unless the decision of the United States Supreme Court in the Trans-Missouri case shall be reversed, made illegal all agreements among railways in regard to the charges to be exacted for their services.

Thorough investigation will show that the competition sought to be perpetuated is not only useless but wasteful, extravagant, and the fruitful source of unjust discriminations among individuals, communities and classes of traffic. In order to maintain effective competition of this kind large expenditures which would otherwise be unnecessary are incurred. These include commissions to those who can secure for the lines paying them traffic which might traverse other routes; expenses for advertising, in excess of that necessary for the information of the public; the cost of maintaining soliciting agents and agencies; of duplicate operating organizations, etc. If better methods could be substituted and these wasteful expenditures could be eliminated the savings effected would accrue to the benefit of the public in the form of reduced charges for railway services.

As competition of the kind under discussion can be effective as to but few individuals, including those only who reside at points served by two or more railways, its result is to reduce the charges at the competitive points, while the general average is kept up by relatively high rates on non-competitive business, thus producing an almost infinite series of unjust discriminations against points served by single railway routes. Stations on local and branch lines are discriminated against and even the less important shippers at competitive points are unjustly treated as a result of the struggle for traffic. Agreements in regard to rates constitute the only remedy and such agreements, in order to be effective, must provide for the division of traffic.

There is another form of competition that has controlling effect upon railway charges which, owing probably to the attention demanded by that already discussed, has almost escaped the attention of students and does not appear to have been adequately discussed or thoroughly understood. All economists consider cost of transportation as a part of cost of production, but little further attention seems to have been paid to railways as productive agencies. There are two kinds of production, that of form utilities and that of place utilities. As modern industry is organized the former would be amost useless without the latter. Cheap transportation has caused a high degree of localization and specialization of the production of form utilities and enhanced the relative importance of the production of place utilities. Very large percentages of the textile production of Massachusetts, of the cotton crop of Texas, and of the wheat crop of the Dakotas would be almost worthless without transportation. But if the production of place utilities is important to the producer of form utilities the latter is equally necessary to the transporter. To encourage and foster industry in the territories contiguous and tributary to their lines is the necessary and constant aim of railway managers. They do so by combining with producers of form utilities and the object of each of these combinations which are as numerous as the independent productive agen-

cies of the first class, is always to market the commodities produced so as to secure the cost of production and as much more as practicable. The competition of these combinations in which railways are compelled to enter, and in the sacrifices of which they frequently are compelled to accept unduly large shares, keeps railway rates down to the lowest level that will meet operating expenses and pay the minimum amount which will be accepted by those who furnish capital for the creation of railway property. The amount of this minimum is dependent upon several elements including risk and the amount of risk in railway enterprises is usually fixed, in some degree, at least, by the extent of competition of the first class. If society wishes cheaper transportation it should eliminate such competition in order that the railways, as producers, may compete most effectively.

HOW TO INCREASE EXPORTS AND HOW NOT TO. By EDWARD ATKINSON, Boston, Mass.

IN this paper Mr. Atkinson presented the statistics of exports of American goods showing that the power of purchase rested mainly with those states and nations in which the advancement of science has led to the increase of purchasing power, the United Kingdom of Great Britain and her colonies buying from us fifty-five per cent of what we had to sell, Germany, France, the Netherlands and Belgium a fraction under thirty per cent, while Italy, Spain and all other European states were able to purchase a little less than six per cent. It therefore proved that those states and nations in which modern science had been in greater or less measure applied had the power of purchase with respect to ninety per cent of our entire exports of the last fiscal year, in amount more than $1,100,000,000, while all the Latin American states of South and Central America, Mexico, and the West Indies, not British, had been able to purchase from us but a fraction over six per cent, the people of Asia, not British, two and three-fourths, of Oceanica, not British, a fraction over one-half of one per cent: Africa, not British, a fraction under a half of one per cent. Thus it appeared that the British colonies only had the power of purchasing from us eleven per cent of all our exports, while the entire world outside of Europe or of British control had the power of purchase of less than ten per cent.

According to ordinary business rules it would therefore seem to be for the interest of this country to promote closer trade relations with our kith and kin beyond the sea rather than wasting time and effort in entering upon militarism and imperialism in the futile effort to increase trade with the non-machine-using nations.

THE INHERENT VICE OF LEGAL TENDER. By EDWARD ATKINSON, Boston, Mass.

HE dealt with the question of legal tender, saying good money needs no act of legal tender. Only bad money calls for an act of force or legal tender to make people take it whether they want it or not. He held that the so-called free coinage act proposed in 1893 was nothing but a pretext. Coinage consists in the manufacture of round discs of metal certified in weight and quality by the stamp of the government. That exhausts the word. Free coinage could be admitted of gold, silver, copper or nickel if that were the end of the matter. The vice of the free coinage policy is hidden under the name of legal tender. Legal tender acts were born in fraud and have been nursed in corruption ever since. That is the record from the dawn of financial history to the present day. All legal tender acts or decrees with the exception of those relating to subsidiary coin have been intended for one of two purposes. They have either been the acts or decrees of absolute rulers or despotic governments for the purpose of cheating their people and defrauding them of their earnings, or else they have been acts for the collection of a forced loan in the conduct of war, like our legal tender acts of 1861–'62; or else for the collection of a forced loan for the purchase of silver under the Bland and Sherman acts which is now resting in noxious desuetude in the vaults of the treasury of the United States.

Money is necessary to the conduct of commerce. Commerce is an exchange of products or services for mutual benefit.

Who invented money? No one knows. All exchanges must first have been in the nature of barter. When men met, each of whom had something to spare which the other did not want at that time, money came into use. It is one of the great inventions of the world corresponding to the wheel, the making of fire, the spinning of a cord.

The oldest records of commerce are those of the Assyrians, Babylonians and Egyptians. They were engaged in a great commerce. They used different kinds of money — money made of gold and money made of silver, both metals passing by weight. They also used oxen, sheep and other cattle. Less advanced nations have used shells, leather, skins, stock-fish and many other articles; always something of constant demand either for ornament or for use. Money was invented to meet the necessities of trade before there was any law or legislation in the sense in which those words are used. It came into use by custom and out of customs originated decrees, ordinances and laws. By and by laws were perverted under acts of legal tender to cheat the masses of the people. Coinage was invented long after money had begun its work, nuggets of gold and silver passing by weight. The names of all the early coins are names of weight. Every contract to sell or deliver goods was a barter of coin passing by weight. Every contract for the purchase of goods was a barter of coin

passing by weight for a measured quantity of goods. We shall never solve the difficulties and confusion of our coinage and our currency until we come back to this true conception of a double barter of one thing for another: goods for coin; coin for goods; credit money convertible into coin by weight serving its true purpose.

The whole international commerce of the globe is conducted in that way by the weight measure of a pound sterling. There is no lawful coin known as a pound sterling. We bartered American cotton, wheat and other products of our farms last year for $800,000,000 worth of gold coin measured by weight in pounds sterling. If any farmers are fools enough to want silver money, or cattle money, or skin money they can get it; silver in Asia, cattle money in Africa, fur or skin money in Nova Zembla. The free coinage of silver of full legal tender is almost grotesque in its folly and audacity, asking farmers in the great Mississippi valley, eighty per cent of whom are free of any mortgage whatever and are thus the creditors of the world, to give their principal customers in Great Britain the power to force British silver costing twenty five cents an ounce or less upon them under an act of legal tender at one dollar and twenty-nine and a half cents an ounce, or sixteen to one. The force of folly could no further go.

Mr. Atkinson then dealt with the history of coinage. He pointed out that the great bimetallist, President Francis A. Walker, defined money as "that which passes freely from hand to hand." Therefore his theory of bimetallism was totally opposed to his own principle; the only kind of money that passes freely throughout the world being money made of gold.

Cernuschi defined good money as "the coin of which the bullion is worth as much after it is melted as it purported to be worth in the coin; all else bad money." The only kind of coin in the world's market which is worth as much after it is melted is gold coin.

The advocates of free silver, bimetallism and greenbacks are alike misled by the fallacy that all money is the creation of law and that it is law which gives it monetary force. They have never studied economic history. They appear to be incapable of dealing with the great facts of commerce. They substitute theories and fallacies which have been exposed over and over again in the history of the world and which have brought disaster whenever and wherever adopted.

Mr. Atkinson quoted from the words of Pelatiah Webster, the patriot merchant of Philadelphia, who resisted the issue of the Continental legal tender paper money, traced all the disasters which it caused and which was nearly fatal to the patriot cause and who, after it died, wrote of its evil effects and the benefit derived from its final death. Yet, as Mr. Atkinson said, there are men apparently of sincerity and ability that propose to renew all these evils by forcing fiat money on an unwilling community; concluding as follows:

The vice of legal tender is that it deprives men of their right to free contract; that it enforces fraud upon an unwilling or ignorant community

whether its members desire to cheat each other or not. It is a bar to the integrity of a nation and to the people alike. The sincerity of its promoters can only be justified at the expense of their repute for common sense.

WHY NOT TRY A NORTH AMERICAN ZOLLVEREIN? By RICHARD T. COLBURN, Elizabeth, N. J.

THE United States with a population of seventy-five millions, and growing, is a good customer to the rest of the world, enjoying a high average wage, a lavish scale of living, and a consumption of imported articles of an annual per capita value approximating $11 of which but little is reëxported either in original form or worked over. The value of this market is well understood abroad, and by none more so than our neighbors on the North, the Canadians. They, however, profess to be unwilling to enter our political federation to secure this desirable trade; and there is no reason why we should press them in that direction; they can offer us only a limited market of five millions as against seventy-five, in which there is hardly a reciprocity; but instead they resort to diplomatic stratagem and unneighborly spite to worry us into compliance, going so far as to favor the mother country by an abatement of twenty-five per cent on manufactures they might obtain from us. This is as strictly within their right as for us to exclude their seal furs. The meeting in Quebec offers an opportunity to put the commercial, industrial and transit relations of the two peoples on a more equitable, permanent and automatic basis, one which would be economically and diplomatically, if not also strategically, advantageous to both parties. (1) We may let the alternative of annexation bide its own time; several causes loom up to make it more desired by the Canadians than by ourselves. (2) The alternative of retaliatory tariffs can be held in reserve as a last resort. (3) There remains that of a common tariff, or customs union for the whole of North America, or so many of the colonies and nations as choose to enter it, allowing the tariff rates against the rest of the world outside to be uniform, and framed after due hearing of delegates from parties in interest, by the Committees of Congress at Washington. Duties to be collected at seaports and retained by the member importing. A like community of freedom might open the fisheries assimilating commerce, mining and fishing industry to the present service in railroad transit, postage currency, news, etc. The "bonding privilege" could remain, but it might be abolished with advantage.

The professions of good will coming from Britons and Canadians, may be stamped with sincerity by taking this practical concrete form. The gain or loss to the Treasury of either would not be great, if we offset the saving to be effected by abolishing custom houses and surveillance along a common frontier line of over four thousand miles. But fiscal advantages are trifling compared with the greater security for peace and

amity between kindred peoples. Moreover, the United States has it in its power to compensate both Britain and Canada abundantly for any sacrifice of dignity or profit there may be to either. The conflicting claims in Eastern Asia, the Nicaragua Canal, the danger of a hostile coalition threatening dismemberment of the British Empire may bring us to the side of Britain. We cannot permit even Canadian territory to pass by conquest, permanently, into hands other than our own. For this security, hitherto lightly regarded, Canada can afford to be very self-denying. British capital invested on both sides of the line must be benefited greatly by an industrial and commercial unification.

NICARAGUA AND THE CANAL. By Dr. WOLFRED NELSON, F.R.G.S., New York, N. Y.

THE republic of Nicaragua, her resources and geographical position, were discussed at length. Geographically, North America (the United States of America and Canada) occupies a position midway between Europe and Africa on the one hand, and China and India on the other. A completed canal will have an immense importance to the United States, with her new Pacific possessions — the Hawaiian Islands and the Philippines. With the latter or an island of that group, North America has vast possibilities within her reach. When the Nicaragua canal becomes a fact the United States and Canada will be many days nearer the East than England or Europe, certainly twelve or fifteen days nearer.

The Nicaragua Canal, with locks, will have its Pacific entrance at Brito. The section of land between Brito and Lake Nicaragua, when cut, will have several locks — Lake Nicaragua is a magnificent inland body of water, one hundred and twenty-five miles long, with an average breadth of forty miles — the water lane across the lake will end at San Carlos, the latter a mere hamlet, marking the entrance to the San Juan del Norte, or the lake's outlet to the Atlantic Ocean. Part of the river will be used — at the longer two-thirds, the canal will leave the stream, and be cut through land and rock to Greytown on the Atlantic. At Greytown or San Juan del Norte, a harbor will have to be made — it will cost much money, owing to drifting sands and ocean currents.

An engineering work of such a magnitude — within the tropics — must cost considerable money and many lives. If the canal costs $200,000,000 or $300,000,000, what of it? Let the engineering and climatic difficulties be fully estimated before any start is made. Let the many and real difficulties be faced with Anglo-Saxon intelligence and frankness, when all will be well. The practical question arises, Will such a canal, at such a cost, pay? If the canal is built by the United States and England — built under joint control — its cost will be far less than if built by any private corporation. The cost, be it what it may, $200,000,000 or $300,000,000, if guaranteed by the countries named, will be a matter of small moment, if placed in the balance and weighed, to its usefulness to the

United States and England. The money for construction can be had at 3 per cent under a joint guarantee.

The completed canal would make New York, Boston, Montreal and New Orleans manufacturing cities of the first magnitude. New Orleans, on America's vast artery, the Mississippi—now that the river is opened all the year, thanks to the Holt quarantine system, one that protects without interfering with commerce — may become the industrial centre of the new South. Her geographical position places her in the front rank —her nearness to Cuba and the proposed canal — and will make her a strong competitor in the race for manufacturing supremacy.

In my paper on "Cuba: Past, Present and Future," I ventured to forecast Cuba's future, as an American possession, saying, "Cuba and Jamaica are keys to the Gulf of Mexico." Let us recall the fact of the nearness of Greytown; while they are the keys to the Gulf of Mexico, they are on the Atlantic highway and the Anglo-Saxon sentinels of the canal.

To any student of this important and timely theme — 'the time when is now. Let the United States and England see to it that the early marriage of the Atlantic and Pacific takes place. With such sponsors at the wedding, it indeed will be a *fin-de-siècle* event of the first magnitude; it will be a history-making event, a final binding of the bonds uniting the Anglo-Saxon races, one in language, blood, thought and purpose; one in advancing commerce, learning, religion and science.

SOME EFFECTS OF RECENT TARIFF LEGISLATION ON THE IMPORTATION AND THE DOMESTIC PRODUCTION OF SUGAR. By FRANK R. RUTTER, U. S. Geological Survey, Washington, D. C.

THE increased interest and importance of the sugar tariff in recent years illustrate the marked change that has taken place with regard to agricultural protection in theory and in practice. The sugar schedule is indicative of the general character of the tariff acts and the circumstances of their passage. The act of 1890, a surplus-protective measure, reduced duties but maintained protection; it removed the duty on foreign and substituted a bounty on domestic raw sugar, and retained a protective duty on refined sugar. The act of 1894, a surplus-revenue tariff, further reduced duties, mainly along non-protectionist lines; the bounty was repealed and a revenue duty on raw sugar was restored, but an additional protective duty on refined, specific in form, was granted. The act of 1897, a deficiency-protective measure, raised duties and increased protection, imposing a protective duty on both raw and refined sugar.

The duties imposed have been *ad valorem*, specific, combined *ad valorem* and specific, and graduated specific, in form. The last form — a specific duty varying in amount with the purity of the sugar — has the fairness of an *ad valorem* duty without the danger of undervaluation, it yields more constant revenue, and it may be proportioned to home rather than to for-

eign valuation. The *ad valorem* duty, by exempting cost of freight from dutiable value, unduly encourages distant importation, usually of lower grade sugars. Thus under the act of 1894 the proportion of Dutch East India sugar imported largely increased — from an average of 5.2 per cent of the imports not above No. 16 Dutch standard in color, during the fiscal years 1892 to 1894 to 12.2 per cent during the years 1895 to 1897.

The countervailing duties, designed to offset European export bounties, have been successively raised and made more general in the acts of 1890, 1894 and 1897, the last of which imposed a special duty on all imports on which a foreign bounty has been paid, equal in amount to the bounty. As a result, the imports of raw sugar from Germany, which, stimulated by the Cuban war, increased from 8.6 per cent of the total in 1894–95 to 32.0 per cent in 1896–97, fell off in 1897–98 to 5.1 per cent.

Protection to domestic cane sugar is measured by the duty on raw sugar; to domestic beet sugar, the product of which enters into direct consumption, by the duty on refined; and to the refining interests, by the difference between the two duties. The differential in favor of refined sugar during the fiscal years 1888 to 1890 was about ninety-eight cents on one hundred pounds of refined; under the act of 1890, about fifty-one cents; under the act of 1894, about forty-two cents; and on German refined under the act of 1897, including the countervailing duty on refined, about fifty-five cents.

The bounty law stimulated both beet and cane sugar. Beet sugar, forming less than 2 per cent of the domestic product in 1890, increased to 6.8 per cent in 1894, and in 1897 constituted 12.3 per cent. In the case of cane sugar, the bounty law facilitated the transition that was taking place in the method of manufacture from the primitive open kettle to the vacuum pan with its far-reaching economies. The bounty law favored high grade sugar; it seemed to give promise of permanence; and it offered protection against free Hawaiian sugar, as a duty did not.

Every change in the tariff has, for a time, adversely affected the prices of Louisiana sugar as compared with the New York prices of foreign sugar.

THE ETHICAL FUNCTION OF THE ECONOMIST. By JOHN DAVIDSON, M.A., D.Phil., University of New Brunswick, Canada.

THE discussion of how far ethical considerations should enter into political economy is in itself a confession of weakness and the new school of economists seek to remove the ground for the old attacks on the dismal science by introducing into economics all sorts of extraneous, ethical, sentimental and sociological considerations. But little has been gained by these concessions and economics is even less authoritative than in the hands of the classical economists. The remedy has been sought in the wrong way by making such admissions. The question is not, How far

shall ethical considerations enter into political econony? but, What is the ethical function of the economist?

Economics forms part of the data of ethics and the function of the economist is to make the data as complete as possible. His work therefore is analytic and he discusses the conditions of one of the dominant human motives. With the synthesis of action he has nothing *qua* economist to do. All intelligent action is based on knowledge of the situation and that action is right which fits the conditions of the case. General moral rules are statements of the analysis of the most permanent elements of situations and economic laws are but more detailed analyses of the conditions of one of the motives on which men act. That economic motives are too often allowed too wide a scope is true, and it is also true that the economist is apt to magnify his calling and try to dictate what shall be done as well as the means to do it. The economist however merely indicates the proper steps to take assuming the end is desirable. Whether it is desirable or not is a question for the individual agent. Economic laws are therefore merely hypothetical imperatives and their application depends on the individual's decision whether or not the hypothetical imperative is also a categorical.

The economist is naturally, like all scientists, liable to the bias of the specialist; but when he proceeds to the synthesis of action and dictates what shall be done as well as how to do it he is transcending his function. But he remains a man and a citizen and he has the right as such to make his own synthesis of action. Yet he does this not as an economist. Special knowledge however is no bar to this further social duty, and although he is liable to the bias of the specialist yet his specialization is deliberate and not that unconscious specialization which we call prejudice. Other things being the same the possession of knowledge is an advantage in action; and the ethical function of the economist is to contribute to that knowledge of the conditions of action which must necessarily precede and be the condition of all intelligent and therefore all right action.

THE DEVELOPMENT OF COLONIAL POLICY. By JOHN DAVIDSON, M.A., D. Phil. (Edinburgh), University of New Brunswick, Canada.

WHILE colonies may be conveniently classified in one of two groups, those capable of responsible government and those unfit for democratic institutions, the difference is simply one of the methods of administration. The end of colonial policy remains the same in both groups: namely, to treat the colony as an end in itself and not as a mere appanage or convenience to the mother country to be exploited either for trade or for offices. This is the only basis of a colonial policy which has been approved by experience and while the idea of monopoly may be natural to the political mind, history shows that disaster follows monopoly and that the evolution of a sound policy is from monopoly to freedom. This is the course of English

colonial policy, the only successful policy in history. Though it is now the basis of English policy it has not always been; and the development has been the work of centuries during which by force of events the policy has been moulded. English colonial policy is divisible into three periods: (1) to the war of Independence; (2) to 1850; (3) to the present day; and each period has its own characteristics.

The first period has been studied and its characteristics are well known. In it England followed to a certain degree the policy of monopoly and the end was disaster. The sole use of a colony was the monopoly of its trade; but this commercial restriction was the sole badge of dependency. Politically they were not restricted. The second period is a complete reversal of the first. Politically the colonies that remained were not free, commercially they were less restricted than in the first period and they received in addition certain preferences in the English market. The Napoleonic war prevented statesmen from fully applying the lessons of the war of Independence and for thirty years the policy was governed by temporary exigencies. After the war the task of reform was taken up. First the commercial restrictions were removed. Then the political restrictions, imposed to prevent what Pitt called the recurrence of the late unhappy events in America, was lifted. The compensation lingered longer until the colonists forgot that the preference was in compensation for restriction. But the growth of free trade ideas made the removal of the colonial preferences inevitable.

The third period begins with the triumph of free trade in England which left the colonies free from external restrictions. But it was a triumph of free trade rather than of freedom and England had intended that the colonies should also be free trade countries. The colonies objected and the result was conflict with the colonial office. The colonists appealed to the principles of responsible government and step by step won the sometimes sullen acquiescence of the home government to their complete and absolute control of their own affairs and in 1897 they achieved the last step when their imperial initiative was admitted.

The result is that colonial policy is not on a definite footing of freedom. The colonies are administered for themselves; whether or not for themselves depends on circumstances. They are not in any way sacrificed to the interests of the mother country and are never regarded as a market to be monopolized.

THE ECONOMIC STATUS OF THE NURSE. By MRS. HELEN DAVIDSON, Fredericton, New Brunswick.

NURSING is an instance of an occupation which during the last half century has risen from being the last refuge of the incompetent or the work of the sentimental and philanthropic, almost to the dignity of a profession, making high demands on moral character, demanding long

and rigorous training and highly specialized knowledge, and having a recognized, though as yet unformulated code of professional ethics. The development has taken place as the complement of the medical and surgical advances of the century. The specialized skill of the physician and surgeon requires specialized skill in nursing that the results may not be lost.

There are many problems at present arising for consideration. The profession is hardly at all recognized. There is no recognized standard of efficiency and the result is overcrowding and irregular employment. Nurse is a term which is made to include women of all degrees of competency — the graduate of the finest hospitals in the largest cities, the graduate of small provincial schools where experience is limited and training of nurses slovenly, the graduates of special hospitals such as children's, fever, etc., and the individual whose services have after a period been dispensed with at her training school who yet claims to have been one of its nurses. The public is largely responsible for this confusion of things that are different and the differentiation of the grades will be the result of increased public intelligence. As it is, the nurse from the better schools is doing what she can to establish a distinction between herself and the comparatively untrained. Partly to overcome the difficulties of irregular employment and to find a substitute for nursing registries which are too often "blackmail" institutions demanding a percentage of all earnings, alumnæ associations have been formed and a National Alumnæ Association is in process of organization. This latter association which is formed of the graduates of schools above a certain standard promises to undertake the solution of certain problems of great economic importance. It proposes to remove conditions which demand work night and day till the case is ended, thus sapping the vitality of the nurse and necessitating a rest of four or five months in the year. It proposes to control the training of nurses by excluding all trained in small and efficient schools. It hopes to establish a code of professional ethics. The result of its work, if it is successful, will be the organization of a profession on an economic basis where different degrees of efficiency will not be confounded, where the conditions of work will be human, where regularity of employment will be so far as possible secured, where a guarantee will be offered to the public that when they pay the comparatively high prices which nurses command they will not be the victims of a confusion of terms receiving something else than highly trained services for their money. One certain result will be the disappearance of the small school. Small hospitals are a necessity. Small schools in connection with small hospitals are an economic nuisance and result in an imposition on the public. Another result will be that the public will become convinced that skill in nursing is not an intuition but the result of training; and the newly philanthropic and sentimental person will not be able to foist herself upon the public whether as a Red Cross nurse or as a district visitor to the degradation of other workers whose name she usurps and the confusion of a public service.

AMERICAN INDUSTRIAL EXPOSITIONS, THEIR PURPOSES AND BENEFITS. By Dr. MARCUS BENJAMIN, U. S. National Museum, Washington, D. C.

THIS paper showed the development of industrial expositions from the fairs of the middle ages and gave brief historical summaries of the three great international expositions held in New York, Philadelphia and Chicago. The interstate expositions were then discussed and the leading characteristics of those held in New Orleans, San Francisco, Atlanta, Nashville and Omaha pointed out and the great benefits conferred by each on the adjacent territory indicated. The paper also discussed how the selection of exhibits might be improved and when such a condition was achieved the awards in the way of medals and diplomas would be more appreciated. Incidentally the work of the jurors was referred to and it was shown that a more careful selection of exhibits would also greatly facilitate the work of making proper and satisfactory awards.

SCIENTIFIC BOOKMAKING. By CHARLES W. FELT, Marlboro, Mass.

THE scientific manufacture of books by literary and mechanical workers is being constantly improved and the most promising advance in the near future may be expected from mechanical improvements in the line of the type writer which will enable the author to make at small cost several copies of his work so as to secure as far as possible before printing the criticisms of experts, and to supply the printer with "copy" as nearly perfect as possible.

Then the reliance upon authority, frequently the defective memory of a single person, defective even in quotations, calls for investigation, and is liable to end at dense ignorance. There is quite too much readiness to print the words of some well-known men who may be ill-informed. Frequently, under present methods, the man who does not know is taken as authority, and the man who knows is treated with practical contempt simply because he is not widely known.

Some conspicuous books contain many errors, and we must find a remedy. We need a Bureau of Criticism competent to make suggestions that will command public respect, and aggressive enough to call upon publishers in extreme cases to recall defective editions and replace them with books that will better serve the public. Such a Bureau should encourage authors to consult experts, and publishers should be induced to take all possible precautions, especially to gather corrections to be made in future editions, even though years may elapse and change of publishers intervene.

Many of us have occasion to remember thankfully the welcome aid of printers and publishers. But unfortunately we have suffered much from books which are a disgrace to our boasted civilization, and sometimes these books come to us bearing the names of individuals from whom better

things should be expected. One difficulty is that most literary men have but a limited conception of their own knowledge of words. A good knowledge of words, like a good name, is more to be desired than great riches. Men commonly have an idea of the number of dollars they possess, but few have an intelligent idea of the number of words they have at command. A few years ago I examined this point carefully and can show that most intelligent people have a vocabulary of 15,000 to 20,000 words. I have printed but not published two books of 250 and 550 pages containing twenty-five samples of selected English and their analysis into words, being an original investigation, in due time to be completed and published as a contribution towards scientific bookmaking.

The early hieroglyphics written on wood, stone or metal were in form suited for such materials. With the use of paper the letters became in time suited for the pen. Now we want letters suited to typography. The type-writer teaches us that it is better to leave an equal space for every letter. The I should receive an addition and the W and M should be reduced. Cheapness requires fewer characters, and a new alphabet is wanted to take the place of capitals and lower case. Telegraphic alphabets have been introduced in this century, and for three centuries stenographic alphabets have been multiplied by hundreds. Now there is need of a distinctively typographic alphabet to lighten the labor of author and printer, and I am led to believe that a character can be devised that will serve the purpose of both capitals and small letters.

A PLEA FOR MANUAL AND INDUSTRIAL TRAINING IN HORTICULTURE. By Prof. WILLIAM R. LAZENBY, Ohio State University, Columbus, Ohio.

THE ECONOMIC POSSIBILITIES OF CUBA. By ROBERT T. HILL, U. S. Geological Survey, Washington, D. C.

[THIS paper was printed as a chapter in "Cuba and Porto Rico, with the other West Indies," by Robert T. Hill, Century Company, New York, October, 1898.]

THE ECONOMIC VALUE OF GOOD ROADS. By A. W. CAMPBELL, C.E., Department of Agriculture, Ontario, Canada.

THE science of economics has many branches, some of which may be more prominent than that which pertains to common roads, but none are more worthy of careful consideration. There was a period belonging to the history of the Roman Empire when the importance of roads was so fully recognized that the control of road construction belonged to the Emperor. In later years, we find an Emperor of France, the first Napoleon,

whose power overshadowed the whole of Europe, devoting his attention to the construction of roads. These roads of France and Rome, it is true, were railways and military routes as well. And if the importance of the common roads appears to have lessened in the mind of the people, it is only because attention has been diverted for a time to the construction of those magnificent lines of steel, which form a network over the continent from the Atlantic to the Pacific, and are an indelible record of the march of civilization and prosperity.

With respect to the actual monetary economy of roads, it is difficult to arrive at exact conclusions. In different localities, where the extent of population, the average length of haul, the topography, products of the land, and other circumstances vary, the financial returns from road improvement must also differ. Moreover, statistics of a reliable nature on which to base conclusions are difficult if not impossible to gather. We can only reach approximate results which are, however, from their magnitude, very convincing.

The financial returns to the farmers have been represented in a number of ways, one of the chief of which is in cheaper haulage. This may be in the shape of larger loads, the result of easier traction. For example, from experiments conducted for the French Government we learn that to move a wagon and load weighing together one ton of 2240 pounds, at slow pace, the traction force on a level Telford road surfaced with exceedingly hard stones, was forty-six pounds; on a level macadam roadway of small broken stones, sixty-five pounds; on a gravel road, one hundred and forty to one hundred and forty-seven pounds; on a common earth road, two hundred pounds. That is, nearly four times as much could be drawn on a perfect Telford road as on an earth road, provided the surface were level. If the load had to be lifted up an incline or lowered, this ratio would vary.

The economy of good roads may, from the above, be expressed in a reduced number of horses, practical illustrations of which we have in England and Scotland where it is rare to see a farm load drawn by more than one horse, so perfect are the roads. If the number of farm horses could be reduced by one-half, or, making every allowance for farm requirements, say one-quarter, the saving would be enormous. The number of farm horses employed in the United States, I do not know, but in my own Province of Ontario, reports of the Bureau of Statistics show that in 1896 there were 434,384 working horses. This number does not include the unbroken horses nor breeding mares, but represents the number actually used for work. Estimating the cost of horsekeeping at $60 a year, which is, I believe, a fair average, this would mean an annual saving of $6,515,760 — no inconsiderable amount to a population of about two million.

Another basis of estimating the loss may be that of more rapid haulage and travel which would bring us, if we estimate time at a money value, to an enormous amount. In this connection I am reminded of a statement made by a Canadian member of Parliament who has used the road to a considerable extent, that he had lost one-half his life by being compelled

to travel over bad roads. The statement may be overdrawn, but it contains a fund of truth from which startling conclusions may be deduced.

The saving in haulage may be estimated with respect to the extended period in which it may be carried on. In this connection I would quote from my annual report of 1897 which included an estimate on this basis: "From the middle of October until the end of December, and from the first of March to the middle of May, a period of five months, by far the greatest part of Ontario is mud, ruts and pitch-holes. This may vary somewhat in the more northerly and more southerly part of the Province, but is the general rule of the average year. Of this period of five months, there are at least two months when the roads are practically impassable for loads. From the middle of November until the middle of December; from the middle of March until the middle of April, the agricultural trade of the country is practically cut off. For the remaining three months of the five, the roads are barely passable. To this may be added January and February for part of the Province, those southerly counties of Western Ontario where sleighing exists for two or three weeks at the most. If we include a winter month (and during the winter there are many days when travel is shut off) there are six months of the year when travel and teaming in Ontario are retarded by bad roads. Part of that time, at least two months as pointed out, country traffic is practically impassable. If during those two months fortune favor us with a few days when teaming can be done, there are plenty of days during the remainder of the year to make two months of impassable roads over the Province generally.

Statistics, as previously stated, show that in 1896 there were 434,384 working horses owned by the farmers of Ontario. Let us assume, and the assumption is a very moderate one indeed, that one-half of these horses, which would otherwise work, are prevented from doing so for two months or sixty days of the year, and that their teamsters are also left in idleness.

This means that for sixty days, 108,596 teams and teamsters are idle. The price generally paid is $3 a day for a team with a driver, and is a proper estimate of the value to the country. The direct financial loss daily, therefore, is at least $325,788, and for sixty days, $19,547,280. At 3% this is the interest on a capital of $651,576,000. The result of an estimate on this basis shows that Ontario could afford to invest $8,000 on every mile of road in the Province. A large proportion of the road mileage included in this estimate is but very little travelled, and it is safe to say that one-third of this amount, or about $200,000,000 — which is approximately the sum invested in the steam railways of the Province — would convert Ontario into one of the best paved countries in the world.

Further estimates may be made with regard to the possibilities of longer hauls, as in France, where, because of the excellence of the roads, teamsters can compete with the railways in drawing goods several hundred miles. There is an additional saving in the wear of wagons, harness and horse-flesh which, trifling in appearance, is, in the aggregate,

no small amount. In this it has been estimated that the life of a horse would, by means of good roads, and consequently less overwork, be extended by a year — a very doubtful assumption.

A further division under which the financial returns to the farmer are to be considered, is the increased value of farm property. This increase in the value of farm property arises in the first instance from the greater profits accruing from farm property as the result of the saving in the cost of transporting farm produce. There are two ways of rendering any business more profitable. One is by increasing the price obtainable for the article produced; the other is by decreasing the cost of production. Cheaper transportation by means of good roads, means in effect that the decreased cost of production and consequent increase of profit will guarantee a larger and readier investment in the farm.

The value of the farm is further enhanced by the increased opportunities that arise through good roads. The farmer is not impeded at any season of the year in the sale of his produce and can in consequence reach the market when prices are highest. Good roads create a market for perishable produce, fruit, certain vegetables, milk, which, if it cannot be taken to the consumer in the town or city with the least possible delay, is unsalable or can be disposed of only at a reduced price. Good roads bring farm lands, ten, twenty, forty miles away, into available distance of a city market; whereas, on the other hand, one mile of really bad road may render otherwise fertile land useless. Distance with respect to the farmer and the market is not measured by miles so much as by the time and labor it takes to transport his produce.

Among other branches of agriculture which demand good roads, dairying may be pointed to as a specific instance. Cheesemakers tell us that, in order to secure the best quality of cheese, the milk should be transferred to the factory as quickly as possible, with the least possible amount of jolting, and that the cheese should be manufactured in large quantities. The effect of changing the roads from bad to good in a dairying district is at once plain. The milk is carried over smooth roads expeditiously from the dairy to the factory, with the least possible exposure to the sun, and least amount of churning in transit. The milk can be drawn from a larger area, the cheese made in larger quantities, factories fewer but larger and manufacturing expenses thereby reduced, while a more uniform quality of cheese is produced. An an illustration of this, and one which is directly in point, I would refer to the largest creamery in the world at St. Albans, Vt. St. Albans is a railway centre, and because of this the creamery was located there in order that the milk could be brought hundreds of miles by rail. By manufacturing in large quantities they can afford to pay the cost of railway transportation and still realize a profit. Railways are merely smooth, hard roads with easy grades, making carriage cheap; and what railways have done for the creamery in St. Albans, good roads would do on a smaller scale for each creamery and cheese factory.

The value of farm land is further increased by the social privileges

which good roads bring. One of the great drawbacks of country life is its isolation. Man is a social animal, and without the privilege of meeting his fellows, he cannot attain his highest development. In spite of the healthful surroundings of farm life, its pure air, its abundance of the best and purest of foods, statistics show that the insane asylums contain a greater proportion of farmers' wives than of any other class of the community. The cause assigned is the monotony to which they are subjected, the absence of all stimulant to thought and lively interest such as society creates. Why it should be the farmer's wife rather than the farmer is of course to be attributed to the lesser strength of constitution which the woman possesses, the nature of her work which is so largely indoors, and further the fact that the man has, in marketing his produce, more frequent journeys to the neighbor or town. Good roads bring the farmer and his family nearer to the neighbor, to the town, the school, the church and the public meeting.

The value of the farm is increased by increasing the attractions of country life, and encouraging a greater liking for it. To-day the tide is from the country to the city. Were country life what it can be made by means of easy access and communication, the tendency would be to draw the city folk to the country. In England, a country house is as necessary to every man of means as is his town house. The beautiful country-side of England is its greatest charm: it is beloved by the people and has done much to install that patriotic affection which every Englishman and Scotchman has for the motherland. Cuba is lost to Spain because she did not join the good roads movement.

To the townspeople, good roads have a distinct economic value. Good roads are of vast benefit to commerce. Agriculture is the foundation of a large proportion of the trade of this country. As agriculture reappears, so will business prosper. Better home markets and better foreign markets for the farmer will bring more money into the country. This means increased sales for the merchant and a greater demand on the manufacturer. Bad roads, besides the retarding influence on the country by lessening its ability to compete in foreign markets, cause produce to be marketed, not as the demand or prices require, but as the roads will permit. The result is that the market is alternately glutted or starved; there is either unnatural excitement or depression. The stores are either deserted, or extra clerks have to be employed. Railways either cannot provide cars to move the contents of their granaries, or else their cars and their crews are in idleness. Such fluctuations cannot but be hurtful. Trade is sensitive and is easily deranged. When farmers can market their produce easily and regularly they have money with which to enable the merchant to turn over his stock and meet his obligations at the bank. The banks are thereby enabled to do business to greater advantage, and the whole machinery of commerce moves with less friction, less useless expenditure of energy. The stringency which so often takes place in the wet seasons as the result of bad roads; the bad roads of Christmas time which so often bring disappointment to the merchant; the resulting fail-

ures which act and react on all classes of society are of well-known occurrence. The condition of the roads is a matter of frequent comment in commercial reports and is one of the most potent factors influencing the barometer of trade.

Then, too, just as the turning of the townspeople from the city to the country is of benefit to the country, so it is of benefit to the town that its inhabitants should not cluster into the least possible space. By means of good roads, the business man can live in the country and each day go into the city or town to transact his affairs. He and his family can thereby inhale a purer moral atmosphere as well as a purer air, largely oxygen and ozone. He can surround himself with better sanitary conditions and live more nearly in accord with Nature's laws.

While the returns from good roads cannot be reduced to dollars and cents with any degree of accuracy, the cost of road construction and repair is a matter of mathematical solution. We can tell to within a few dollars what certain work on a road will cost, but it is not possible to tell exactly what the profit on the investment will be. From what has been previously outlined, the profits arising are, however, plainly very great to the country folk, the townspeople and the state as a whole. Great Britain spends twenty-five million dollars a year on roads, France almost twenty millions. These are both countries of good roads, both countries of the finest business instinct. The object would appear to be to provide good roads at any price. The United States and Canada are each spending considerable sums on roads — on bad roads. Of course there must be some exception made in favor of a few states, notably Massachusetts which is everywhere quoted as the foremost state in this most laudable enterprise.

There is a most important feature of roadmaking which must not be overlooked. The road must not merely be a good road with respect to actual construction, but it must be good in view of the service it will render and the cost. A road must be looked at in the same way as any other investment — with a view to the profits. It was in this respect that the Romans failed. Some of their roads are still in use, after nearly two thousand years of service. They were strong and durable, but built at such a prodigal expenditure as no nation could afford — a prodigality which became characteristic and eventually resulted in the decay of the Empire. If we are to receive the greatest benefit from good roads, we must carefully weigh the service they will render with the cost, keeping the balance as much in favor of the former as possible. In England, in France, in Germany, in some sections of Canada and the United States where population is concentrated and largely urban, the limit of expenditure on the best of Telford and macadam roads can be reached. The greater part of this continent, however, is sparsely settled, and there is as great, perhaps greater, need of farm roads adapted to simple requirements and circumstances and costing $1000 or less a mile, as there is for roads costing $10,000 a mile. In the building of the cheaper class of roads, there is need of the greatest skill, for if the expenditure available is small, there is the more need to apply it carefully.

Who should pay for country roads? Should the whole cost rest on the farmer whose property is reached by the roads? Or should the towns and cities pay a portion? Should road construction be performed at the expense of the local municipality, or should the state participate?

The last mentioned alternative, state aid, is commonly adopted, and is the means whereby towns and cities, all classes of people, assist in paying the cost of road construction. Towns and cities are obviously benefited by country roads. Without roads, towns and cities could not exist. Being benefited, there is no reason why towns and cities should not assist in meeting the cost.

There are other reasons why state aid meets with approval. It appears to be the only means whereby roads of a better class can be built. They are the best of means in educating the public with regard to the value of roads. Good roads are their own best advocates. Not only do they educate as to the value of roads, but they instruct and are object lessons in the best principles of road making, and thereby assist in improving the roads which remain under the exclusive control of local councils.

The difficulty of arriving at exact conclusions with regard to the economic value of good roads, because of the absence of complete and reliable data, has been commented upon. There are additional impediments to our reaching a complete solution in the fact that the different benefits of good roads will combine, act and react upon one another so as materially to alter relative conditions. Ease of traction, longer hauls, more rapid travel, fewer horses, saving in wagons, harness and horse-flesh; increased rural population, less congestion in the cities, increased land values, greater profits on the farm; better business and social facilities,—will all unite toward the one end in enabling us to live well and more wisely. The people of the American continent have, to an abnormal extent, turned for their ideals to city life. The greatest ultimate benefit which good roads can confer is to bring us back from the towns which men have made, where brick walls, stone pavements, and odors of filth, have shut out remembrance of field, forest, stream, sky, and Nature's God —this is the most desirable of economic results.

A SUFFICIENT SOCIAL PRINCIPLE. By Rev. CHARLES A. EATON, Toronto, Canada.

SOCIAL and political science exist for the alleviation and advancement of mankind. Social thought at present confused and contradictory. The need is to discover the all-embracing principle upon which modern society is founded and to present another principle great enough to take its place,

1. *Things first* is the principle now obtaining as basis and standard of organized society. Illustrations and instances in proof from industrial and social life of America, England and Germany. The sad condition of men, who are fifty years of age, in many callings. The preponderance of prop-

erty over manhood as the object of legislation. The existence of monopolies for private profit primarily rather than public service.

2. *Things first* is proving itself unequal to the task of further supporting human society. Negative evidence—increasing difficulty of living; competition run mad; literature and art degraded to a money-making trade; journalism a commercial venture; all realms of life commercialized. Positive evidence — attempts to counteract evils by various philanthropies; free education; changed attitude of the church; multiplicity of reforms; organization of labor.

3. *Manhood first*, the principle sufficient for the foundationing of society. Theoretically this is the dominant principle now in religion and education.

This principle applied as a working hypothesis to the slums, to education, to the unemployed, to legislation, to industrial organization, to inventions, to art and literature.

4. "Manhood first," declared by revelation and science to be the natural order.

5. Objections considered.

In order to become supreme, this principle must be energized by life. In all real progress conscience is the instrument and the energy is moral and spiritual. We need to reëxamine the primal forces of our social organism.

A CATALOGUE OF SCIENTIFIC AND TECHNICAL PERIODICALS, 1665-1895. By H. CARRINGTON BOLTON, Ph.D., Washington, D. C.

[This work, which was read by title only, was published by the Smithsonian Institution in 1898 as No. 1076 in its Miscellaneous Collections.]

The titles of the following papers were presented but the papers were not read.

THE FORMATIVE PERIOD OF A GREAT CITY: A STUDY OF GREATER NEW YORK. By WILLIAM H. HALE, Brooklyn, N. Y.

ON THE UNITED STATES' ALLEGED POLICY OF IMPERIALISM, SO CALLED, AND IN CONNECTION THEREWITH, SOME REASONS FOR AND AGAINST THE PROPOSED ANGLO-SAXON ALLIANCE. By W. LANE O'NEILL, New York, N. Y.

THE TRANSPORTATION PROBLEM. By JOHN S. WILLISON, Toronto, Ont.

THE GOLD STANDARD AND THE UNEMPLOYED. By CHARLES B. SPAHR, New York, N. Y.

THE STUDY OF POLITICAL ECONOMY IN CANADA. By S. MORLEY WICKETT, Ph.D., Toronto University, Toronto, Ont.

THE AGRICULTURAL STATISTICS OF ONTARIO. By CHARLES C. JAMES, Deputy Minister of Agriculture, Toronto, Ont.

CANADIAN FORESTS AND THE PAPER INDUSTRY. By THOMAS SOUTHWORTH, Clerk of Forestry, Toronto, Ont.

AN ACCOUNT OF THE

FIFTIETH ANNIVERSARY MEETING.

REPORTS OF THE

GENERAL AND PERMANENT

SECRETARIES.

AN ACCOUNT OF SUCH FEATURES OF THE FIFTIETH ANNIVERSARY MEETING AS ARE NOT INCLUDED IN THE REPORTS OF OFFICERS.

As was foreshadowed by the unusual character of the invitations to the Association to hold its Fiftieth Anniversary Meeting at Boston, both on account of their extreme cordiality and from the fact that they came from so many high sources, including the Governor of the Commonwealth of Massachusetts, the Mayor of the City of Boston, the Presidents of Harvard University, Massachusetts Institute of Technology, Boston University, Tufts College, the Boston Society of Natural History, the Massachusetts Horticultural Society, the Appalachian Mountain Club, the American Academy of Arts and Sciences, and the Essex Institute, no effort was spared to make this anniversary meeting a perfect success.

The Local Committee, which was promptly formed, included many persons prominent in science, literature, and educational affairs, and was not confined to residents of the City of Boston, but included the Presidents of Radcliffe College, Williams College, Amherst College, Massachusetts State Agricultural College, Clark University, Wellesley College, the College of the Holy Cross, Mt. Holyoke College, Worcester Polytechnic Institute, and Smith College, in addition to the Presidents of institutions named among those sending invitations. The preparations made by this Local Committee were undertaken on a scale suitable to the character of the occasion and probably not before equalled in the history of the Association.

The meetings were held in the buildings of the Massachusetts Institute of Technology, in the Lecture Room of the Boston Society of Natural History, and in the building of the Harvard Medical College, the general sessions being held in Huntington Hall.

Handbooks and guide books of an elaborate nature had been prepared and were placed at the disposal of the Association. These included a Handbook of the Scientific Institutions of Boston, a Guidebook to Harvard University, and a Guide to localities illustrating the Geology, Marine Zoölogy and Botany of the vicinity of Boston.

The program of excursions was an exceptionally fine one, including trips to the Middlesex Fells, to Salem, to Boston Harbor, to Marblehead, Nahant and Cape Ann, to the Metropolitan Water Works at Clinton, to the Arnold Arboretum, to the Blue Hill Meteorological Observatory, to Wellesley College, and to Lexington and Concord. Excursions at the conclusion of the meeting were offered by the Local Committee to the White Mountains, to Plymouth, to Provincetown, to Woods Holl, to Newport, and to the Lawrence Experiment Station.

The social features of the week included general receptions by the Trustees and Officers of the Boston Museum of Fine Arts, by the Trustees and Officers of the Boston Public Library, by Governor and Mrs. Roger Wolcott, by Mrs. W. B. Rogers, by Mrs. John C. Phillips, and by Mr. and Mrs. John L. Gardiner. Mayor Josiah Quincy also gave a dinner to the principal officers and guests of the Association at the Union Club. Several of the Boston Clubs including the St. Botolph Club, the Algonquin Club and the Technology Club were thrown open to members of the Association and the meetings of the Council were held in the rooms of the last-named organization.

The proceedings of the opening and closing sessions of the Association, including the addresses by Governor Wolcott, Mayor Quincy, and President Crafts, are given in the detailed report of the General Secretary which follows, and the details of attendance and the work of the Sections follow in the report of the Permanent Secretary. Two of the especially notable features of the meeting, however, Salem Day and Cambridge Day, are not noticed in these reports and deserve especial mention here.

SALEM DAY.

In accordance with the plans of the Local Committee, made in consultation with the President-elect of the Association, Wednesday, August 24, was set aside as "Salem Day." A special Salem

Committee was organized under the chairmanship of Hon. Robert S. Rantoul, with Mr. Henry M. Batchelder as Secretary. The members of the Association reached Salem both by boat and by train on the morning of the 24th and assembled in the pavilion at the Salem Willows where addresses were made by President Rantoul of the Essex Institute, Ex-President E. S. Morse of the Association and Mayor Waters of Salem.

President Rantoul's address was cordial in the extreme and at times humorous, somewhat at the expense of other places. He said he was not going to welcome the Association to Salem, for a gathering of people who had, during the waking hours of three mortal days, heard nothing but welcomes from everybody they had come in sight of from His Excellency down, must be welcomed enough and be eager for some other diet.

When our eloquent executive, said the speaker, welcomed you to Massachusetts he welcomed you to Salem, for Salem was the beginning of Massachusetts, and there were years when one might almost say that the Massachusetts Bay Colony was Salem. We are older than Boston, and naturally in many things ahead of Boston. Our friends at Boston are just now indulging themselves in a good deal of jubilation over a certain subway which they seem to have just completed, and which they seem to regard as a signal triumph of their engineering genius. But, my friends, said the speaker, for sixty years past it has been impossible to get out of Salem by rail to the eastward without passing through a subway successfully operated all these years under the very heart of the city. So in other matters. Our Boston friends will give you to understand that they had more or less to do with bringing on the Revolution, and will tell you a good deal about the breaking up of Boston town meetings by British authority, and about Lexington and Concord and Bunker Hill and John Hancock and Samuel Adams. This is all very well: it pleases them and does no harm to anybody. But Mr. Webster, in his immortal oration on the death of Adams and Jefferson, says that the vote which cut Massachusetts adrift from the home government was passed at Salem town house just a year to a day before Bunker Hill. And it happens that, only two months later,— a good many months before any Boston town meeting had been interfered with by British authority, Gage marched his Fifty-ninth regiment up to town from their camp on

this very Salem neck, in an abortive attempt to disperse with bayonet and powder and ball a Salem town meeting. Timothy Pickering had proved as intractable as Samuel Adams.

And let me say in this connection that while the British carried their point at Lexington and Concord and captured Bunker Hill, in each case accomplishing what they were sent forth to do, they had planned in February, four months before Bunker Hill — two months before Lexington and Concord — just such a raid on Salem, and had come to Salem on that errand, with the Sixty-fourth regiment stealthily embarked from Boston Harbor, but after a little parley with some of our old sea-dogs, they concluded to return to Boston without the guns they were sent to capture and reported to Gage that those Salem folk were too much for them, and that they had accomplished nothing.

Professor Morse spoke delightfully of the former visits of the Association to Salem — the first in 1869, when it passed a week here, and next in 1880 when it visited Salem for a day and dined at Kernwood. His remarks called up pleasant memories to many who had been present on one or both of those occasions. Mayor Waters gracefully extended the hospitality of the ancient borough.

A luncheon in the adjoining pavilion followed. The menu was printed in what may be termed zoölogical and botanical Latin and was the source of much amusement. After the luncheon the members in attendance divided into groups and under the guidance of sections of a large local committee of the Essex Institute and Peabody Academy of Science, visited different points of interest in accordance with a printed itinerary furnished by the local committee. The main headings of the itinerary included educational institutions, points of geological interest, colonial architecture, old cemeteries, points of interest in connection with the history of witchcraft in New England, the City Hall, North Bridge and points connected with Hawthorne's life in Salem.

The Essex Institute had just commemorated its first half-century and the fiftieth aniversaries of the two associations thus coincided. The Institute, therefore, extended an especial welcome to the Association and a committee of ladies was in attendance at the rooms of this old and important organization to entertain visiting members of the Association. At four o'clock the steamer returned to Boston where the Association reunited in the evening to listen

to illustrated lectures on the Metropolitan Water Supply by the Hon. Henry H. Sprague, and on Transit in Boston, by the Hon. Geo. H. Crocker.

CAMBRIDGE DAY.

By invitation of the President and Fellows of Harvard University the Association was the guest of Harvard University on Friday, August 26, which was designated in the programs as "Cambridge Day." A committee to welcome the Association, composed of members of the faculty and ladies, was arranged by President Eliot. All departments of Harvard University were thrown open for inspection and the officers of the University were present at the different laboratories and museum to give special explanations. As above indicated an elaborate and beautifully illustrated guide book of the University had been prepared by a committee of the Faculty for the use of members of the Association. Section H held a largely attended meeting in the Peabody Museum. Section G was given a special exhibition in the Department of Botany. The visits to the different departments of the University began at 10:30 in the morning and at 12:30 luncheon was served in Memorial Hall. Restricted receptions were given by several of the officers of the University, among which may be mentioned a tea by Professor Farlow, Vice-president and Chairman of Section G. A general tea was given by the corporation of the University in Memorial Hall at 6 o'clock. In the evening, the Association assembled at Sanders Theatre together with many prominent citizens of Cambridge and Boston. The President of the Association, Professor Putnam, presided, and introduced President Eliot, of Harvard University, who delivered the address which follows under a separate heading. This address was one of so great importance and such widespread interest that the Council of the Association, by special resolution, requested of President Eliot permission to publish it in full.

The meeting, as a whole, was thus a memorable one and fitly marked the fiftieth anniversary of the founding of the Association. Probably no other meeting, certainly none within the recollection of the writer, has been so notable in the attendance of so many of the older and more prominent scientific men of the United States. No less than nine past presidents of the Association took

part in the discussions of the week, and it is a remarkable fact that the distinguished man who presided, Prof. Wolcott Gibbs, was one of the five survivors of the group of scientific men who founded the Association fifty years ago. A painful fact connected with the meeting, which deserves especial mention here, is that Prof. James Hall of Albany, also one of the founders of the Association, died on his way to this meeting.

L. O. HOWARD.

ADDRESS

BY

CHARLES W. ELIOT,

PRESIDENT OF HARVARD UNIVERSITY.

DESTRUCTIVE AND CONSTRUCTIVE ENERGIES OF OUR GOVERNMENT COMPARED.

We have been witnessing during the past five months an extraordinary exhibition of energy on the part of the Government of the United States in making sudden preparation for the War with Spain, and in prosecuting that war to a successful issue. As men of science, or teachers or promoters of science, we have a special interest in the lessons of the war; because the instruments and means used in modern warfare are comparatively recent results of scientific investigation and of science applied in the useful arts. Moreover, the serviceable soldier or sailor is himself a result, not only of moral inheritance and instruction, but of training in the scientific processes of exact observation, sure inference, and accurate manipulation. It is not the linguistic side of school training which makes the effective soldier or sailor; it is the scientific side. His vocabulary may be limited though expressive, and his grammar false; but his eye must be true, his judgment sound and prompt, and his hand capable of using instruments of precision.

Many suppose that chemistry, mathematics, and physics are the only sciences which have contributed to the resources of modern warfare. This is far from the fact. Biological science is an important contributor. The first-relief package, which every soldier carries, is crammed with surgical knowledge which the world waited

for till the last quarter of the nineteenth century. The hospital ship Bay State is full of appliances for the care of the sick and wounded which are new within twenty years, and have all resulted from scientific discoveries and inventions made in times of peace, and for purposes the opposite of warlike. Physiological science has really arrived at valuable conclusions with regard to the soldier's diet — the indispensable foundation of his effectiveness — conclusions which relate to portability, nutritiousness, and adaptation to different climates; though it must be confessed that these conclusions do not seem to have affected as yet the practice of the United States Commissary Department.[1] Financial science is also

[1] The ration of the United States soldier is a liberal one in comparison with that of other armies; but if the Commissary Department avails itself of the option to issue pork or bacon, it is a ration ill-adapted to a warm climate. Nevertheless, good cooking would make the American ration an acceptable and wholesome one.

WAR RATIONS:

	Quantity allowed d i y	
BRITISH SOLDIER IN INDIA:		
Meat with bone,	16.00	ounces
Bread,	16.00	"
Potatoes,	16.00	"
Rice,	4.00	"
Sugar,	2.50	"
Tea,	0.71	"
Salt,	0.66	"
Total,	55.87	"
GERMAN SOLDIER:		
Bread,	26.50	"
Fresh or Raw salt meat,	13.25	"
or Smoked Beef, Mutton, Ham, Bacon, or Sausage,	8.82	"
Rice or ground Barley,	4.41	"
or Peas, Beans, or Flour,	8.82	"
or Potatoes,	53.00	"
Salt,	0.90	"
Coffee roasted,	0.90	"
or Coffee raw,	1.00	"
UNITED STATES SOLDIER:		
Fresh meat,	20.00	"
or Salt Beef,	22.00	"
or Pork or Bacon,	12.00	"
Bread or Flour,	18.00	"
Potatoes,	16.00	"
Peas or Beans,	2.40	"
Rice,	1.60	"
Sugar,	2.40	"
Coffee raw,	1.60	"
Salt,	0.25	"

a contributor of prime importance; since success in war depends more and more on the command of money and credit. To this war with Spain we owe the most effective revenue bill, or rather the only comprehensive revenue bill, the country has had within a whole generation.

It cannot be doubted, then, that the energy put forth by our Government for the immediate purpose of capturing or destroying Spanish vessels, forts, towns, and war material, and incidentally killing, wounding, and starving Spaniards, has been a great exhibition of power in applied science, and as such must commend itself especially to the attention of this Association. I hear already a protest against the thought that men of science can have any special interest in war,—war the supreme savagery, the legalization of robbery and murder, the assemblage of all cruelties, crimes, and horrors, set up as an arbiter of international justice. I recall the indictment set forth by Charles Sumner forty years ago in his address on the War System, "that this trade of barbarians, this damnable profession, is a part of the War System, sanctioned by International Law; and that war itself is hell, recognized, legalized, established, organized by the Commonwealth of Nations for the determination of International questions!"[1] This is the jurist's and philanthropist's view. But the man of science has another view of war. He regards it as the worst survival of savage life, still occasionally unavoidable because of other survivals of the savage state, such as superstition,

[1] "'Give them hell,' was the language written on a slate by a speechless dying American officer. 'Ours is a damnable profession,' was the confession of a veteran British General. 'War is a trade of barbarians,' exclaimed Napoleon in a moment of truthful remorse, prompted by his bloodiest field. Alas! these words are not too strong. The business of War cannot be other than a trade of barbarians — a damnable profession; and War itself is certainly Hell on earth. But consider well — do not forget — let the idea sink deep into your souls, animating you to constant endeavors — that this trade of barbarians, this damnable profession, is a part of the War-system, sanctioned by International Law; and that War itself is Hell, recognized, legalized, established, organized by the Commonwealth of Nations, for the determination of International questions!" (War System of the Commonwealth of Nations: an address by Charles Sumner, before the American Peace Society, at its Anniversary in Boston, May 28, 1849. Boston: Pratt Brothers, 37 Cornhill, 1869. Stereotype Edition. In pursuance of the above vote of our Society, several large editions were issued; but, thinking that a performance of such signal ability ought to have a still wider and more permanent circulation, we asked permission to stereotype it. Mr. Sumner kindly consented; and in preparing this edition, he has made no alteration in any principle or argument from the original address, his views, like our own, having experienced on the question of Peace and War, no change from any events of the last twenty years. Geo. C. Beckwith, Corresponding Secretary. Boston, Jan., 1869.)

passion uncontrolled, and lust of wealth and power. He recognizes the fact that war makes a temporary and local hell on earth, and that all its characteristic activities are destructive; whereas all the normal activities of a free government should be constructive, and intended to promote the good of its citizens and general civilization; but he does not accept Sumner's dictum in his oration of 1845 on "The True Grandeur of Nations," "There can be no war that is not dishonorable." He recognizes that occasional war, and therefore constant preparedness for war, are still necessary to national security, just as police, courts, prisons, and scaffolds are still indispensable to social order and individual freedom in the most civilized and peaceful states. Moreover, the man of science perceives that, while the immediately destructive objects in war are savage and barbarous, the instrumentalities and forces used in modern warfare are closely akin to the great constructive agencies and forces in human society. The battleship is, to be sure, the most complex and the cruelest machine yet constructed by man; but all its parts, except its armament and its armor, are not only applicable in works of peace, but have actually been wrought out for peaceful constructive purposes. The organization and disciplined skill which make possible the equipment of great bodies of soldiers within a few weeks, and their transportation to distant lands with incredible speed and safety, are the same sort of organization and skill needed in every great productive industry; and the mechanical and electrical engineers, who have become indispensable in warfare, have been developed, not for war, but for modern industries and systems of transportation. The applications of Bessemer steel in war are not its primary uses; its peaceful constructive applications give it its primary value. The application of compressed air for the transmission of power was not invented for the dynamite gun, but for tunnelling and mining. The ammonia refrigerating process was not invented for hospital uses in war, but for domestic and commercial cold storage. No nation can now succeed in war which has not developed in peace a great variety of mechanical, chemical, and biological arts. The normal activities of these arts must and do tend to advance humane civilization. Their application to the destructive cruelties of warfare is abnormal. Yet, inasmuch as they are applied in war with a prodigious energy and intensity, it may well be that the acute horrors of even the shortest war may

have a lesson for the long normal periods of peace. The destructive activities of the Government of the United States are abnormal and rare; but they are intense, and they attract in a high degree the attention and interest of the people. I therefore wish to call your attention to some of the lessons which this unusual energy of the Government in war suggests in regard to its normal activities in times of peace.

One further introductory explanation seems to be needed for the sake of clearness. There is a class of *a priori* social philosophers who would not be at all content with this moderate claim that times of war may have useful lessons for peaceful times; for they believe that the virtues bred and the habits established in war alone make possible the assured progress of society during peace; and that, therefore, occasional wars are to be welcomed as renovators of society, which during peace tends to corruption, luxury, and enfeebling vices. Now men of science, so far as I have observed, generally think that this doctrine just reverses the real order of cause and effect. They do not consider the martial virtues — courage, endurance, loyalty, and the willingness to subordinate self-interest to the interest of clan, tribe, or nation — to be the supreme and ultimate objects towards which the human race must struggle on. They regard these virtues as the elementary, fundamental, preliminary virtues, which can be cultivated in man's savage state, and so become the stepping-stones of his moral advance; but they know, on the demonstrative evidence of both history and natural history, that these virtues may coëxist with cruelty, rapacity, and lust, and an almost complete indifference to both truth and justice. Civilization, in their eyes, means the adding of justice, truth, and gentleness to the martial virtues, an addition which does not necessarily involve any countervailing subtraction. The civilized man should be as brave, enduring, self-sacrificing, and loyal as the savage, and should also be just, truthful, magnanimous, and gentle. The warlike virtues are those of the hunter, and war is a chase with man the prey; but as man rises in the scale of civilization, he is less and less the nomad and the hunter. Truly, it is not war which prepares men for worthy and successful lives in times of peace. On the contrary, it is worthy life in time of peace on the part of individual men or a nation of men, which prepares for success in war; and this principle is quite as true of men in the sav-

age state as in the civilized. The winning tribe in savage warfare is that which in peace lives habitually a simple, hardy, robust life, loves the chase and daring sports, travels fast and far afoot, and subsists at need on what it can find on the way, or carry with it in the rudest methods. In civilized warfare, that nation will be successful which produces plenty of healthy, vigorous, intelligent men, who have added to the ancient martial virtues a moral quality which free institutions can best develop, namely — individual initiative and self-reliance — and have acquired skill in a great variety of useful arts. Do we not all believe that the normal activities of peace under free institutions are the best possible, though not the only necessary, preparation for inevitable war, and that such normal activities of the nation never need to be purified or uplifted by avoidable war? Nevertheless, we may also believe that some lessons for times of peace can be drawn from the prodigiously stimulated activity of the Government and the sacrifices of the people in time of war.

The first important inference which may be drawn from the experience of our Government and people during the past five months is anthropological — it is the permanence of the martial virtues and their commonness. In any vigorous race these virtues may fairly be called inextinguishable. A whole generation has passed since this country has been at war, just as a whole generation passed between the war of 1812 and the Mexican War; and yet courage, endurance, and patience were promptly exhibited by hundreds of thousands of our young men. The extinction of the soldierly qualities is not at all to be feared in a robust race inhabiting the temperate zone, which cultivates manly sports, and pursues on land and sea all the occupations which require the maintenance of a watchful struggle against adverse powers of nature, or the utilization of natural forces of mysterious and formidable intensity. Civilized society is always maintaining a perilous conflict with natural forces, which ordinarily serve man's purposes, but sometimes try to overwhelm him. Fire, the greatest of man's inventions, and his humblest servant, suddenly breaks out into destructive fury; wind ordinarily fills his sails, turns his mills, and refreshes the atmosphere of his cities, but now and then in spots sweeps from the surface of the earth and sea all man's works — crops, buildings, vehicles and vessels. The mineral oil

which every night lights so brilliantly the humblest homes in every clime occasionally kills the ignorant or careless user, or sets a huge city in flames. Any single-minded worm or insect will be too much for man, unless man knows how to set some other creature of one idea at destroying the first invader. How small is the range of the thermometer within which man can live with comfort or even safety! A change of a few degrees below or above the normal range sets him fighting for his life. This conflict with external nature is the great school of mankind in courage, persistence, patience, and forethought; and mankind never needs any other. The professional soldier may be softened, and perhaps corrupted, by a long period of peace; for in peaceful times he may have nothing to do, or at least his occupation may be so slight and dull as not to keep his physical and mental powers at full play; but a citizen soldiery, when free from the horrible activities of war, returns promptly to the labors of peace, and escapes the dangers to which a professional soldiery is exposed. It is, then, the regular pursuits and habits of a nation in times of peace which prepare it for success in war; and not the virtues bred in war which enable it to endure peace.

The second lesson to be drawn from the recent experience of the nation in war is the immense value of long-prepared, highly-trained public service. The instant efficiency of our navy is a striking demonstration of this principle, which of course needs no enforcement before men devoted to science; but does need to be brought home to the great body of our people. The war teaches that though a navy can be extemporized for the purposes of transport and blockade, for fighting purposes the trained naval expert is the invaluable man, whether in command or behind a gun or in the engine room. The preparedness of our Regular Army for immediate service and the comparative unreadiness of the militia, even in those states which have paid most attention to volunteer military organization, enforce the same lesson. Would that the plain teaching of this short war in this regard might sink into the minds of our people, and convince them of the immense advantages they would derive from a highly-trained, permanent, civil service in every branch of the public administration!

Another lesson of these pregnant months relates to a principle which underlies our form of government, yet is often seen but

dimly by portions of our people. I refer to the principle that the Government of the United States should do nothing which any other visible agency — state, city, town, corporation, or private individual — can do as well. This seems a strange principle to be enforced by the action of our Government in time of war, since the Government has a monopoly of that hideous activity; but this war has brought out in a very striking way the fact that, when it comes to the pinch, the source of victory is in the personal initiative of each individual commander and private soldier or sailor. When all preparation is made, when all appliances have been perfected and brought together, in the particular thicket or mined strait in which the work of the moment is to be done, it is the perceptive power and moral resolution of the individual which command success. In warfare as in industries, the automaton counts for less and less, and the thinking, resourceful individual for more and more. The automaton is the natural result of despotic institutions, civil and religious; the resourceful, initiating individual is the natural product of free institutions, under which the citizens are as little restricted as possible in the development and training each of his own will-power and capacities. To secure this fundamental advantage of free institutions. as many fields of activity as possible must be left open to the individual, and to voluntary associations of individuals. If the Government enters a field which individuals, or voluntary associations of individuals, could till, it diminishes by so much the range or reach of the great school of self-governing freemen, namely, the school of creative and constructive industry under liberty and with responsibility. Is it not a wonderful thing that the invention of more and more destructive weapons, like the long-range magazine rifle and the machine gun, which have made impossible close formations, and forced every modern army to imitate what used to be called Indian warfare, should bring out so strikingly, as this recent war has done, the immense superiority of the disciplined freeman to the trained automaton? A firing line is now composed of detached men, each seeking cover at every moment, and all using smokeless powder, that the exact position of the line may not be revealed to the enemy one thousand or two thousand yards away. The enemy is invisible, and there is none of the excitement of personal encounter. The individual soldier is not supported on right and left by bodily

contact with comrades, and the nearest officer may be a long way off. Under such circumstances each man must do his own fighting, and success depends on the courage, skill, and judgment of the individual soldier. The maxim, "In time of peace prepare for war," means, therefore, vastly more than it used to. It no longer refers chiefly to the provision of vessels, forts, and weapons, but rather to the bringing up of generations of young men trained by school, college, political life, and the great national industries to habits of self-direction and of disciplined coöperation. This bringing up is best secured under free institutions which leave everything possible to the initiative of the citizen.

This principle — that government should do nothing which any other agency can do as well — being admitted and established, the next question to be considered is whether the legitimate activities of our Government in time of peace, activities directed toward constructive and wholly beneficent objects, should not be increased. On this point I cannot help thinking that the lesson of the war is plain and convincing. It is undeniable that our people have rejoiced in the exhibition of power which the Government has given during this war. We have all derived great satisfaction from our Government's display of power, exercised with promptness, foresight, and the sagacious adaptation of means to ends. It is human nature always and everywhere to enjoy such success as the Government has won, even when it costs heavily in blood and money. To have the consciousness of possessing power, and to display the power possessed, is a national gratification. Now, this sort of satisfaction ought to be obtainable in peace as well as in war; so that the power of the United States, displayed in peace for ends wholly constructive and beneficent, ought to be in some measure comparable with the power the Government is capable of displaying for destructive ends in war. Charles Sumner's argument to the comparative cost of the Ohio, a ship of the line, and of Harvard Unisity[1] — a comparison made in 1845 — helped him to the wrong con-

[1] "It appears from the last Report of the Treasurer, that the whole available property of the University (Harvard), the various accumulations of more than two centuries of generosity, amounts to $703,175.

Change the scene, and cast your eyes upon another object. There now swings idly at her moorings, in this harbor, a ship of the line, the Ohio, carrying ninety guns, finished as late as 1836, for $517,888; repaired only two years afterwards, in 1838, for $223,012; with an armament which has cost $53,945; making an amount of $834,845 (Document No. 132, House of Representatives, Third Session, Twenty-seventh Congress) as the

clusion that war is always dishonorable and always to be avoided, and that preparations for war are foolish and criminal. Nevertheless, the comparison was and is highly suggestive, and becomes more and more so as preparation for war and war itself grow more and more costly. Indeed, in one respect the recent war has made such comparisons more effective and interesting; for it has proved that the defence of coast cities and harbors is easier than we had supposed, since the strongest fleets have no formidable powers of offence against them. Comparatively cheap mines, protected by respectable earthworks on shore, cannot be successfully dealt with by any naval forces yet devised. A navy without an army cannot make conquests: and the defence of all important points on a coast can be extemporized at moderate cost. Such comparisons make us desire that the steady energy of the Government for good ends in times of peace be made to bear a better comparison with its intense energy in the spasms of war. How can the United States put forth during the long periods of peace a beneficent power comparable to the destructive power it wields in war without violating the principle of leaving to its citizens every field of activity which they can till to advantage?

If we examine the fields of activity which must perforce remain to the Government, we shall find that they will amply suffice for the exercise of power enough to gratify the most ambitious and the most benevolent citizen of the Republic. Let us briefly survey some of these fields. The first I shall mention is the fostering of commerce. This function obviously belongs to the general Government, which has power not only to regulate, but to annihilate at will, the trade of its citizens with foreign countries. We have, indeed, seen our foreign commerce destroyed by our own national legislation. Now, commerce, foreign and domestic, is the great peace-

actual cost at this moment of that single ship; more than $100,000 beyond all the available wealth of the richest and most ancient seat of learning in the land!

Pursue the comparison still further. The expenditures of the University during the last year, for the general purposes of the college, the instruction of the Undergraduates, and for the Schools of Law and Divinity, amount to $46,949. The cost of the Ohio for one year of service, in salaries, wages, and provisions, is $220,000; being $175,000 above the annual expenditures of the University, and more than *four times* as much as those expenditures. In other words, for the annual sum lavished on a single ship of the line, *four* institutions like Harvard University might be sustained throughout the country!" (The True Grandeur of Nations: an oration, by Charles Sumner, delivered before the authorities of the City of Boston, July 4, 1845. Boston: American Peace Society, 1869.)

maker and peacekeeper, and, on the whole, it is the great enricher of mankind in comforts and luxuries. It deserves on every account the fostering care of a powerful nation, not only for the benefits it confers on the members of that particular nation, but because it tends to bring about the confederation of all races of mankind in the pursuit of a common well-being. The war with Spain has distinctly enlarged the moral outlook of our people. It has presented to them wholly unexpected problems concerning the responsibility of a fortunate people for the welfare of the less fortunate. It has suggested to them that a policy of political seclusion and commercial isolation is not worthy of a strong, free, and generous people; and that such a policy is not the way to the greatest prosperity and the most desirable influence.

Another great field of beneficent activity for our Government is the procuring of just and humane conditions of labor in industries which cannot be carried on within the jurisdiction of any single State, because they necessarily cover several States. For the protection of work-people in industries carried on completely within a single State, State legislation may suffice; but when, as in the case of railroads, the industry must be carried on through several or many States, it is only the National Government that can adequately protect the interests of the persons employed. The great functions of the National Government in this respect are now only beginning to be exercised. In the Ninth Annual Report of the Interstate Commerce Commission on the Statistics of Railways in the United States, a report dated June 30, 1897, I read[1] that in the year

[1]Comparative Summary of Railway Accidents for the years ending June 30, 1896, 1895, 1894, 1893, 1892, 1891, 1890, 1889, 1888:

Year	Employés		Passengers		Other Persons		Total	
	Killed	Injured	Killed	Injured	Killed	Injured	Killed	Injured
1896 - - -	1,861	29,969	181	2,873	4,406	5,845	6,448	38,687
1895 - - -	1,811	25,696	170	2,375	4,155	5,677	6,136	33,748
1894 - - -	1,823	23,422	324	3,034	4,300	5,433	6,447	31,889
1893 - - -	2,727	31,729	299	3,229	4,320	5,435	7,346	40,393
1892 - - -	2,554	28,267	376	3,227	4,217	5,158	7,147	36,652
1891 - - -	2,660	26,140	293	2,972	4,076	4,769	7,029	33,881
1890 - - -	2,451	22,396	286	2,425	3,598	4,206	6,335	29,027
1889 - - -	1,972	20,028	310	2,146	3,541	4,135	5,823	26,309
1888 - - -	2,070	20,148	315	2,138	2,897	3,602	5,282	25,888

(Interstate Commerce Commission; Statistics of Railways in the United States, 1896, page 87.)

1896 the number of railroad employés killed in the service was 1861, and the number injured 29,969, the number of men employed on the railroads of the United States in this year being 826,620. In 1897 the corresponding figures were 1,693 killed and 27,667 injured. These actual numbers equal the casualties of a great battle; but the deaths and injuries occurred in a single year, and are not above the average of the five years preceding. The total number of persons killed on American railroads in the year 1896 was 6,448, while the injured were 38,687, and these figures are not above the average of the five years preceding. In the same year there were killed and wounded in coupling and uncoupling alone 6,614 trainmen, 1,744 switchmen and flagmen, and 328 other employés, making a total of 8,686 killed and wounded in coupling and uncoupling alone.[1] Of the total number of trainmen in the United States, one in every 152 was killed, and one in every 10 was injured during the railroad year 1896;[2] during 1897 one in 165 was killed and one in 12 injured.

[1] Accidents in the United States, 1896, in coupling and uncoupling.

TRAINMEN		SWITCHMEN, FLAGMEN, AND WATCHMEN		OTHER EMPLOYÉS		TOTAL	
KILLED	INJURED	KILLED	INJURED	KILLED	INJURED	KILLED	INJURED
157	6,457	58	1,686	14	314	229	8,457

(Interstate Commerce Commission; Statistics of Railways in the United States, 1896, page 88.)

[2] Comparative summary showing number of employés and trainmen for one killed and for one injured in the United States, for the years ending June 30, 1896, 1895, 1894, 1893, 1892, 1891 and 1890:

YEAR	NUMBER OF EMPLOYÉS FOR ONE		NUMBER OF TRAINMEN FOR ONE	
	KILLED	INJURED	KILLED	INJURED
1896	444	28	152	10
1895	433	31	155	11
1894	428	33	156	12
1893	320	28	115	10
1892	322	29	113	10
1891	296	30	104	10
1890	306	33	105	12

(Ibid, page 96.)

Great battles do not occur every year; but these losses do. Do not these terrible figures suggest that our Government has not yet undertaken to discharge its duty of protecting by legislation large classes of its citizens engaged in indispensable service to the community? The obstacles to the use of automatic couplers are pecuniary alone. On June 30, 1896, only about one-third of the total equipment of American railroads in cars and locomotives was fitted with train-brakes, and only about two-fifths were fitted with automatic couplers. Have we not here a new function for our Government in which the wise exercise of its great power would have far-reaching beneficent results?

As time goes on, it appears that more and more industries have a national scope. Thus, it may be doubted whether the mining of soft coal can be successfully regulated by the separate legislation of single States; for coal mined in Virginia is necessarily in competition with coal mined in Ohio, for example, and the unprotected condition of laborers in Ohio may prevent the adequate protection of coal miners in Virginia. Within a few months New England cotton manufacturers have been startled by the development of the cotton manufactures in the Southern States; and one of the first suggestions of remedy made by the New England operatives was a national law to regulate hours of labor in cotton mills all over the country. This incident simply marks a tendency. Interests common to many States certainly suggest that the common Government has duties in regard to them.

An established function of our National Government is the execution of public works for the improvement of rivers and harbors,—works which redound to the advantage of the localities where they are situated, to be sure, but also to that of the people at large. These works are too often executed in a slow, wasteful manner, which no private person or corporation could possibly afford. As an illustration of bad Government methods, and therefore of the possibilities of improvement in governmental efficiency, I take the Columbia river at the Cascade Gorge. This improvement comprises works on a great lock and on a canal about three thousand feet long, including the lock. The original estimate of the cost was a million and a half dollars, and the work was actually begun in 1878. At the end of 1891, when $1,609,324.94 had been expended on the work, the estimate for its completion was a million and three-quarters dollars. It is not yet finished, after the lapse

of twenty years.[1] In six of the years since the first appropriation was made, Congress made no appropriation whatever. Until 1893 it never appropriated anything like the sum which the engineers reported could profitably be expended in the following year, and even then the appropriation lacked half a million dollars of the money the engineers wanted. The total expended to date is more

[1] Columbia River at Cascade:

Year.	Appropriations.	Amt. Expended including liabilities and contracts.	Available.	Estimated amt. that could be profitably expended in following year.	Estimate for completion from date.
1876	90,000 00		90,000 00		
1877		4,616 65	90,000 00	500,000 00	1,459,136 00
1878	150,000 00	5,854 05	235,383 35	500,000 00	1,524,338 00
1879	100,000 00	44,785 87	329,529 30	500,000 00	1,424,338 00
1880	100,000 00	207,626 83	384,743 43	500,000 00	1,324,338 00
1881	100,000 00	83,269 43	217,116 60	750,000 00	1,224,338 00
1882	265,000 00	133,329 57	133,817 17	500,000 00	1,655,397 00
1883		186,233 53	265,517 60	500,000 00	1,655,397 00
1884	150,000 00	73,586 92	79,284 07	500,000 00	1,505,397 00
1885		133,873 48	155,697 15	750,000 00	1,250,000 00
1886	187,500 00	19,050 74	21,823 67	800,000 00	1,100,000 00
1887		110,445 55	190,272 93	400,000 00	1,850,000 00
1888	300,000 00	77,788 44	79,817 38	500,000 00	1,550,000 00
1889		221,835 26	302,347 59	700,000 00	1,250,000 00
1890	435,000 00	72,858 38	80,512 33	900,000 00	1,115,000 00
1891		234,170 24	442,653 95	1,500,000 00	1,745,000 00
1892	326,250 00	190,650 11	208,483 71	1,419,250 00	1,419,250 00
1893	1,239,653 00[1]	19,398 27	1,588,736 60		
1894		330,984 95	1,564,338 33		
1895		630,000 00	From Bookkeeper's accounts.		
1896		427,001 28			
1897		342,248 72			

[1] Sundry Civil Act of 1893,— "not more than ½ to be expended during the fiscal year ending June 30, 1894."

Total expended,	5,007,742 00
Original estimate,	1,459,136 00
Total expended with interest up to date of 1897 at 4%,	5,889,000 00

than five millions of dollars, not counting interests on expenditures which have stretched over twenty years. In the meantime not a particle of benefit has accrued to the population on the Columbia river or to the nation at large. The delay and waste have been caused by scanty and intermittent appropriations, involving frequent suspensions of work and the deterioration of an expensive plant.[1] The cost of the work has been greatly enhanced by the necessity of renewing the plant, and recruiting anew at short intervals the whole force of work-people. If a vigorous corporation had undertaken the work, it could have completed the job within six years, and would thereafter have enjoyed a good income on the money invested. It is impossible for the nation at large to take satisfaction in grand works so feebly conducted. Such a process impairs, rather than increases, the self-respect of the nation; for everybody perceives that it is a stupid and discreditable process. Whenever a public work must be completed before the country can derive any benefit from it, the Government should prosecute the work with all the dispatch consistent with thoroughness of

[1]Extracts from the reports of U. S. Engineers in charge of the work.

Report of Major James, 1885: "In conclusion, I will only add that if the necessary funds can be afforded, I can open this work for navigation inside of two years, and that every year saved in the opening of navigation through the Cascade Mountains will save to the masses of people affected a sum approximate to the whole cost of the work."

Captain Powell, 1887: "Operations had been generally suspended, from want of funds, for several months previous to August, 1886. . . . . The estimate of cost for completing the canal with the single lock, carefully revised during the year and based on the costs of work done, gives a total in round numbers of $1,850,000. The increase over the original estimate results principally from previously uncounted expenses from suspension of work; the severity of the climate and difficulties of the situation at the Cascade Gorge, were, I judge, not sufficiently considered. . . . On account of small and uncertain appropriations the opening of the Cascade Canal will require several years."

Major Thomas H. Handbury, 1888: "For all works of this character, where the improvement to be effected must be completed before any advantage can accrue to commerce, it does seem that the policy of small appropriations running through a long term of years enhances enormously their ultimate cost."

Major Handbury, 1890: "On the 5th (of July) active work was resumed and continued until November when it was discontinued on account of unfavorable weather and a scarcity of funds."

Major Handbury, 1891: "The estimated amount yet to be appropriated for completing the work is $1,745,500. If this amount were available now, so that the work from this time forward could be pushed to the full extent of our arrangements and the capacity of the plant now provided, it is within the range of possibility, under ordinary circumstances of weather, to advance it so near completion that boats could be regularly passed through the lock by the end of the year 1892; but this is not the case; . . . . "

execution. This single instance illustrates the opportunities for immense improvement in the conduct of the operations of our Government on public works. Already, there are some examples which indicate that better times are in store for us in this respect. Thus, in 1884, estimates of $3,710,000 were submitted for clearing out the mouth of the Columbia River by dredging and constructing jetties. On June 30, 1896, this work was practically finished at a cost of two millions of dollars, favorable circumstances and prompt, continuous work having effected a saving of a million, seven hundred thousand dollars.[1] The rapid erection of the Library of Congress under the direction of General Casey within the original estimates is another hopeful example.[2] The self-respect of the nation is enhanced by every public improvement which is well planned and well executed, and then turns out to be of public benefit proportionate to the expenditure. The cost of clearing the mouth of the Columbia river was not so much as the cost of one armored cruiser; but it is a permanent work of daily utility, the beneficence of which is without alloy.

To illustrate further the directions in which the beneficent expenditures of our government might reasonably be increased, I now invite your consideration of certain comparisons between items of military and naval expenditure which the Cuban war has forced on our attention, and the cost of some government establishments which are of special interest to the Association. The annual cost of the Lighthouse Establishment, on the average of the five years from 1893 to 1897, inclusive, was three million dollars. The cost of maintaining naval vessels in commission during the year

[1] Extract from Report of Major Handbury, 1891: "Receiving reasonably large appropriations, the officer in charge has been enabled to provide a plant commensurate with the importance and difficulties of the work in hand, and has used this to good advantage. The work has been well organized and pushed forward on business principles, as all large Government work must be if economical results are to be expected. The rock and other materials have thus far been obtained at reasonable figures, and the employés have taken a commendable interest in the success of the project and rendered faithful service. This could not have been done had the work been overshadowed with the constant dread of disorganization on account of limited and inadequate appropriations."

[2] The law of October 2, 1888, put the whole charge of the construction into the hands of General Casey, Chief of Engineers, U. S. Army. In March, 1889, Congress appropriated $5,500,000, in addition to $745,000, a balance of former appropriations. It was then estimated that the time of construction would be eight years. The building was completed just within that time; and there was an unused balance of over $50,000.

1897 — a year of peace — was nine millions of dollars.[1] Now, the Lighthouse Establishment is one of the most interesting and useful departments of national expenditure. It has a high scientific quality, and also a protecting, guiding, friendly quality. It renders an unremitting service in storm and in calm, over rough waters and smooth, on both oceans, on the Great Lakes, and on many rivers, and in all the extremes of climate which our widespread country affords. It calls forth in high degree the best human qualities,— intelligence, fidelity, and watchfulness. It ought to be the object of constant interest on the part of the whole population, and of Congress in particular. With our resources and commercial needs, and our thousands of miles of coasts and rivers, the Lighthouse Establishment ought to be the best in the world, as well as the most extensive. Indeed, it ought to be absolutely as good as it can be made, and every promising experiment for the improvement of any single light or of all lights, of any single foghorn or of all foghorns, ought to be promptly tried by the Government without regard to cost. Some other nations and regions of the earth excel us in the proportion of first-order and second-order lights to all lights, and several nations have experimented more patiently and more successfully than we have with

[1] From the U. S. Treasurer's Report:

		1897.	Average for five years 1893–97.
Expenses of the Smithsonian,		$ 127,551 75	123,882 84
" " National Museum,		195,740 14	173,633 80
" " National Zoölogical Park,		67,779 26	54,920 83
" " Fish Commission, general,		428,827 27	362,078 78
" " Colleges of Agr. and Mech. Arts,		1,056,000 00	969,600 00
" " Department of Agriculture,		2,176,530 38	2,030,979 84
" " Weather Bureau,		848,949 81	845,360 07
" Preventing Spread of Epidemic Diseases.		32,677 72	127,619 37
" Protecting Public Lands, Timber, etc.,		92,809 69	90,689 47
" of the Coast and Geodetic Survey,		380,865 52	417,476 27
" " Lighthouse Establishment,		3,390,090 45	3,002,231 77
" " Marine-Hospital,		620,506 90	646,511 81
" " Geological Survey,		422,366 82	382,824 95
" " " Maps of the United States.		65,580 11	58,707 13
" of "Increase of the Navy,"		14,539,911 30	13,680,906 92
Report of the Paymaster General of the Navy.	For new ships,	10,543,373 72	1897.
	Maintaining Ships in Commission,	8,938,549 71	

the electric coast light. There is no doubt that the number of lights and fog-signals might be increased to great advantage, that many more range lights and lighted buoys should be supplied, and that the vessels maintained by the Lighthouse Establishment might be better equipped and better adapted to the service they are expected to render. A government vessel ought always to be of the best possible type, and to be supplied with all the best appliances for its service.

The progress of medical science imposes upon modern governments a new duty towards their citizens, the duty, namely, of protecting them from contagious or infectious diseases. This protection has to be provided by means of inspection stations, quarantines, and other methods proper to secure the isolation of infected persons. The doctrine of State Rights has been invoked in our country to prevent effective inspection and quarantine on our seacoast, and effective isolation in the interior of the country. The assumption by the National Government of an effective control, on the coast and in the interior, over imported infections or contagious diseases, has also been resisted on the ground that national health officers would not be careful of the commercial interests of single ports threatened with the invasion of disease, or actually suffering therefrom; whereas State or municipal authorities would always bear in mind the commercial and industrial interests of the afflicted places. Such arguments against national control of these dangers are narrow and unworthy, and have too long prevented the establishment of an effective National Board of Health. The diseases against which protection is most to be desired are cholera, small-pox, leprosy, and yellow-fever; and these diseases come in at the coast on vessels which are sailing under National authority and regulation. It is impossible to see how an effective control can be exercised over them except by the National Government. The Government has an established agency already, called the Marine-Hospital Service, which has a considerable variety of functions not well indicated by its title. Thus, it examines candidates for the positions of keepers and surfmen in the Life-Saving Service, pilots for steam vessels in regard to color blindness, cadets and seamen for the Revenue-Cutter Service, and renders aid to the Immigrant Service by inspecting arriving immigrants. It is also charged with a certain amount of public health service, but its authority on this subject is not well established, and has often been successfully resisted. It is obvious that the

Marine-Hospital Service is a creditable and useful one, but that it lacks the authority which a National Board of Health should have; and that both its staff and the money placed at its disposal are inadequate to the important ends in view. Now that our Government has driven Spain out from its West Indian colonies, and has assumed possession of Porto Rico, and temporary control of Cuba, an opportunity is afforded of organizing this department, and putting it upon a much more effective footing than would have been possible before. The island of Cuba has been the great source of yellow fever infection; and we now have, temporarily at least, the opportunity of ridding ourselves of this source of danger and dread. At the same time, Congress can reconstruct what is now called the Marine-Hospital Service, and render it, under some other name, a thoroughly effective agent for the protection of the people of the United States from imported preventable diseases. An effective bureau once established would undoubtedly find new opportunities of usefulness to the people. Thus, the pollution of streams occurring within the limits of one State, but affecting the people of other States, is a subject which a National Health Department might very properly deal with; and the disinfection of public interstate conveyances on land and water is another. The community is just beginning to desire the application of effective methods to prevent the diffusion of transmissible diseases. The prohibition of expectoration in public conveyances is a good sign of the advent of better municipal practices with regard to the spread of contagion. The community is also beginning to understand how the industrial effectiveness of the community is diminished by preventable diseases and deaths, and to apprehend the economic aspects of the prevention of disease. The preservation of the public health against the invasion of preventable disease is really one of the great interests of the American people, health and the protection of life to the normal period being infinitely precious to the individual and desirable alike for the happiness and the productiveness of the whole people. Indeed, the public health more directly concerns the public happiness, than does agriculture, mining, trade, or any other of the national activities. The commercial argument for an effective national health bureau is a strong one; yet it is the feeblest of all the arguments for the reënforcement of the existing national health agencies. To remove from American families, or greatly diminish, the fear of death by preventable imported disease would be to confer an immense blessing

on all classes of our people. The progress of medical science has made typhoid fever a preventable disease, and has reduced the mortality in diphtheria to one-third of the former rate. When the record of this short war with Spain is made up, it will appear that one of the few thoroughly discreditable features of the war was the occurrence of numerous cases of typhoid fever in instruction camps within the limits of our own country. The present expenditure of the Government for the Marine-Hospital Service has been about $650,000 a year, on the average for the five years 1893 to 1897. This budget ought to be greatly increased. It would be wholly reasonable for the Government to spend as much on behalf of the public health as it costs to keep three battleships in commission for a year in time of peace, say, one million of dollars. The debates on this subject have been going on for a long time. The cholera invasions of the later forties and early fifties started the discussion. The cholera of 1892 provoked further discussion, and each invasion of our southern coast by yellow fever has increased the public interest in the subject. In Congress, in local boards of trade, and in the communities which have been invaded by epidemic diseases, all aspects of the subject have been reviewed. It is now time for effective action on the part of Congress.

The Life-Saving Service of the United States deserves to be greatly enlarged. The general seacoast of the United States, excluding Alaska, is estimated as 5,705 miles long; but if islands, bays, and rivers to the head of tide-water be included, the estimated length reaches 64,559 miles. This mileage does not include 3,000 miles of lake coast, or nearly 5,000 miles of rivers above tide-water.[1] On June 30, 1895, the number of life-saving stations

[1] In 1889 the Coast and Geodetic Survey at the request of the Light-House Board prepared the following statements of the length, in statute miles, of the general seacoast, and of the coastline including islands, bays, etc., to the head of tide-water:

General seacoast of the United States:	
Atlantic Ocean	2,043
Gulf of Mexico	1,852
Pacific Ocean	1,810
Alaska	4,750
Coastline including islands, bays, rivers, etc., to the head of tide-water:	
Atlantic Ocean	36,516
Gulf of Mexico	19,143
Pacific Ocean	8,910
Alaska	26,376

This mileage does not include the more than 3,000 miles of the lake coast nor the nearly five thousand miles of rivers which are lighted; but it does include the Alaskan coasts, great parts of which are not lighted.

was only 251, and of these 53 were on the Great Lakes, 1 on the Ohio River, and 13 on the Pacific Coast. For the year ending June 30, 1895, the men at these stations gave aid in 675 cases of disaster, the amount of property involved being eleven millions of dollars, and the number of persons involved about six thousand. The mere mention of these figures demonstrates at once the inadequacy of the number of stations. The men employed must possess skill in surf-work and in the use of the various appliances for life-saving, and must be also men of unquestionable courage and good judgment. They are exposed in their routine of duty to many hardships and dangers. They struggle with wind and cold on the shore, and with some of the most formidable dangers of the sea. They must patrol beaches or rock-bound shores in the worst weather, and must be always ready for prompt service by night and by day. They need all the martial virtues; and these virtues are displayed not in killing and wounding, but in rescuing from death and injury. They must have not only individual courage and skill, but discipline and capacity for combined action in moments of great excitement and stress. As the result of the organization of this service, the number of lives lost in proportion to the number of persons on board vessels suffering disaster within the domain of the Life-Saving Service has been greatly reduced. The ratio for the five years 1875 to 1880 was 1 to 65; the ratio for the years 1890 to 1895 was 1 to 95. Shall we not all agree that this noble service should not be limited in its scope by any pecuniary consideration; but only by the probability of rendering service? When the United States undertakes to save life, and in so doing maintains a fine corps of servants whose manly qualities are all exerted for beneficent purposes, it should not consider what the service properly organized costs, but simply how useful it can be made. The appropriation for the fiscal year 1898 was only $1,562,795.

The Department of Agriculture is of comparatively recent creation, dating from 1893. The appropriations made for this department have always exceeded the amount expended, partly because of its newness, and partly because Congress has been disposed to be liberal in this direction. The proper objects of the department are the discovery, study, and development of the agricultural resources of the United States. It is primarily a scientific and technical bureau. Of its twenty-two divisions, seven are admin-

istrative, eight technical, and seven purely scientific. It is distinguished among the departments of the government by having its whole body of servants under Civil Service rules, the only persons not in the classified service being the Secretary, the Assistant Secretary, and the Chief of the Weather Bureau. Its main work is done not in Washington, but at scattered stations all over the country. Thus, there are (1897) outside of Washington 153 observing stations, and 244 stations on the seacoasts and Great Lakes where storm signals are displayed for the benefit of mariners. There are (1898) also 135 meat inspection stations in 35 cities of the country, 28 quarantine stations for imported cattle, 16 stations for inspecting export stock, beside several stations for examining stock for Texan fever. The Division of statistics affords a measure of protection against combination and extortion in buying and selling the products of agriculture. It collects information as to the condition and prospects of the principal crops, tabulates statistics of agricultural productions, and of the distribution and consumption of these products, and issues a monthly crop report for the benefit of producers and consumers. It is obvious that this useful Division tends to check irrational and injurious speculation in food products. The usefulness of the department is beyond all question, whether we consider domestic or foreign commerce, the agricultural industries proper, or the great business of exporting foods. The English government supervises with much care and at large cost the importation, transportation, and marketing of cattle, sheep, and pigs, and of the foods derived from these animals. Why should we be less careful than the English of the welfare of the population in this respect? When we consider the large proportion of our population engaged in industries which this department serves, and the importance of these industries to our national budget, may we not reasonably be surprised that the department is crippled by the parsimony of Congress with regard to salaries? On account of the low salaries authorized for scientific and technical services, the department is constantly losing some of its ablest and best workers. Universities, colleges, and experiment stations carry off the best men. On account of the youth of the department, most of its officers and servants are now young men, who may perhaps be retained for a time at the low salaries authorized by Congress, but are sure to be lost to the service as their age and experience increase. Apart from the Weather Bureau, which is now one of

its divisions, the cost of the Department of Agriculture during the financial year 1896–97, was rather more than two millions of dollars,— about the cost of one day of the war with Spain.

Next to Agriculture in importance to the country comes the mining of coal and the metallic ores. The mineral wealth of the United States, including coal, is immeasurable, and there lie the foundations of all our manufacturing industries, and of the household comfort with which our population is so greatly blessed. One would naturally have supposed that the government of the United States would have been inclined to spend liberally on the discovery and investigation of our mineral resources, and that the Geological Survey of the United States would always have been carefully fostered, and developed as rapidly as possible. Whenever new territory has come into our possession or been newly occupied, we might naturally have endeavored to obtain with the utmost promptness complete surveys of its geological and mineralogical features, in order to bring to the notice of the population the resources of the new areas. Such has not been the history of the Geological Survey of the United States. The expenditure upon it has never been generous, and has often been parsimonious; and large areas of our country have remained for generations unexplored and unmapped. There has been no method of cordial cooperation between national surveys and state surveys, and the geological investigations of the government have generally followed in the wake of private mining enterprises, rather than led the way. For the average of the five years 1893–97 the expenditure of the Government on the Geological Survey and the issue of geological maps was about $450,000 a year, or less than the cost of six hours' war with Spain during the last four months.

In the city of Washington the Government maintains a National Museum, a National Zoölogical Park, and a Congressional Library. All these three institutions taken together do not cost the Government three hundred thousand dollars a year; whereas the English government spends on the British Museum alone about $600,000 a year.

The Weather Bureau of the United States, on which the nation spends less than a million dollars a year, contributes greatly to the comfort and health of the people and to the protection of their property. The warnings it gives of cold waves, frost, hot waves, and high winds, of the coming of heavy rains and the rise of riv-

ers, have a constantly increasing usefulness; yet its number of stations for weather observation is manifestly insufficient, and the number of places at which warnings are conspicuously given is also insufficient. We owe to the war with Spain the first attempt to establish an adequate number of observation stations in the West Indies,— stations which have been greatly needed from the first establishment of the Bureau. The field of observation ought to be much broadened, and its results ought to be more thoroughly and promptly made known. In the year ending June 30, 1897, that is, before the war, the country spent twice as much on mere repairs of naval vessels as it did on the Weather Bureau.

The Coast and Geodetic Survey of the United States has been a great credit to the country, and has a value not only for the protection of commerce, but for the promotion of geographical science, —a value it would be impossible to estimate. It should be maintained in a state of the utmost efficiency, and its results should be at the service of every mariner and merchant. It is a part of the equipment of our Government which has conferred on the United States scientific distinction. Nevertheless, it has often been crippled in its work by lack of steady, timely, and adequate appropriations. Its annual cost for the five years, 1893–1897, averaged $418,000, or only a little over what it cost to maintain in commission the armored cruiser New York[1] for the year 1897.

[1] From a Statement showing the amounts authorized for new vessels under "Increase of the Navy" in each act of Congress from Mar. 3, 1883 to and including the act of Mar. 3, 1893, the objects (ships) authorized, the amounts appropriated, the amounts expended upon each vessel authorized, including armor and armament and the actual total cost of completed ships.

Objects (ships) authorized and dates of acts of Congress.	Amounts authorized for hull and machinery, including hull armor.	Cost of maintenance for one year, including coal, provisions, repairs and pay of officers, crew, and marines.
Act of Mar. 3, 1885 Yorktown	520,000.00	155,435.36
" Aug. 3, 1886 Terror	630,000.00(?)	126,561.47
" Mar. 3, 1887 San Francisco	1,500,000.00	242,845.48
" Sept. 7, 1888 New York	3,500,000.00	391,065.69
" Sept. 7, 1888 Bancroft	260,000.00	82,444.47
" June 30, 1890 Indiana	4,000,000.00	323,695.67
" June 30, 1890 Oregon	4,000,000.00	
" Mar. 19, 1892 Brooklyn	3,500,000.00	

A new department of our Government ought to be at once organized to secure the permanent protection and utilization of the forests on the national domain. The experience of other nations has already demonstrated that well-managed national forest reserves not only pay their expenses, but yield a revenue. The objects of such forest administration are of the utmost importance to a mining and farming population, being, briefly, to ensure a permanent supply of timber, to protect the water-supply in agricultural regions adjacent to the forests, to prevent floods, and to store water which in arid and semi-arid regions can subsequently be utilized for irrigation. The efforts thus far made to protect the national property in forests have not been successful, the greatest destruction being wrought by fire, and by pasturage,[1] but much harm being also

(Note continued from page 610.)

SHIPS.	AMOUNT EXPENDED.				
	For hull and machinery, including hull armor.	For armor for gun protection.	For armament.	For equipment, Bureaus of Equipment, Construction and Repair and Steam Engineering.	TOTAL
Yorktown	548,906.61		156,722.64	62,401.34	768,030.59
Terror	1,234,810.91	144,664.64	133,853.68	64,489.17	1,577,818.40
San Francisco	1,738,257.82		272,876.54	124,168.95	2,135,303.31
New York	3,727,541.29	170,299.03	341,626.43	107,175.64	4,346,642.39
Bancroft	362,505.05		47,559.50	21,217.08	431,281.63
Indiana	4,355,893.53	977,134.02	553,972.48	95,691.45	5,982,691.48
Oregon	4,868,902.47	1,029,591.42	585,598.77	75,412.09	6,559,504.75
Brooklyn	3,621,268.52	323,552.21	341,639.32	137,330.04	4,423,790.09

[1] "Most of the Fresno group (Big Tree lumber) are doomed to feed the mills recently erected near them, and a company of lumbermen are now cutting the magnificent forest on King's River. In these milling operations waste far exceeds use; for after the choice young manageable trees on any given spot have been felled, the woods are fired to clear the ground of limbs and refuse with reference to further operations, and, of course, most of the seedlings and saplings are destroyed.

These mill ravages, however, are small as compared with the comprehensive destruction caused by 'sheepmen.' Incredible numbers of sheep are driven to the mountain pastures every summer, and their course is ever marked by desolation. Every wild garden is trodden down, the shrubs are stripped of leaves as if devoured by locusts, and the woods are burned. Running fires are set everywhere, with a view to clearing the ground of prostrate trunks, to facilitate the movements of the flocks and improve the pastures." (The Mountains of California: by John Muir. New York, The Century Co., 1894, p. 199.)

done by simple stealing of the forest product in districts where there is no adequate policing of the reservations. The experience of Canada has proved, under conditions analogous to those which exist within our own territory, that forest guards and patrols can do much to keep down fires, even in the dryest seasons. The problem in our own country is to procure legislation that will protect the forests, while promoting the occupation by private settlers of land within the districts covered by the reservations which is better adapted to agricultural or mining use than it is to forestry. The opposition to the reservation of forest land which has proceeded from the mining interests is an opposition that prefers the immediate pecuniary interest of a single generation to the permanent pecuniary interest of many generations; for it is certain that diffused mining industries cannot be permanently maintained in regions denuded of timber, except by large companies owning the richest mines, — companies which can support the expense of bringing timber from afar. In semi-arid regions, pasturage is fatal to future forest growth, while in well-watered regions like Oregon and Washington, the injury it inflicts is insignificant; but it is precisely in semi-arid regions that a storage of water for purposes of irrigation is most important. Neither state ownership of forest lands nor private ownership can be satisfactory under present conditions. Private individuals and corporations have an immediate interest in cutting off the timber; and this done, their interest ceases. Wherever forests are cut down for fire-wood, as has happened throughout New England, every tree is cut, and the forest is permanently injured. Many deciduous trees, like the birches and maples, start up again from the stumps, with numerous sprouts, and this sprout-growth remains very inferior to seedling growth. The woods of New England have been seriously damaged by being cropped for fire-wood in successive generations. This may happen in regions where the rainfall is sufficient to secure reforesting; but in arid or semi-arid regions reforesting, when once the original timber has been removed, is extremely difficult or in many cases impossible. Anyone who has travelled through the comparatively treeless countries around the Mediterranean, such as Spain, Sicily, Greece, Northern Africa, and large portions of Italy, must fervently pray that our own country may be preserved from so dismal a fate. It is not the loss of the forests only that is to be dreaded, but the loss of agricultural regions now fertile and populous, which may be desolated by the floods that rush down from bare hills and

mountains, bringing with them vast quantities of sand and gravel to be spread over the lowlands. Travelling a few years ago through Tunisie, I came suddenly upon a fine Roman bridge of stone over a wide, bare, dry river-bed. It stood about thirty feet above the bed of the river, and had once served the needs of a prosperous population. Marvelling at the height of the bridge above the ground, I asked the French station-master if the river ever rose to the arches which carried the roadway of the bridge. His answer testified to the flooding capacity of the river and to the strength of the bridge. He said, "I have been here four years and three times I have seen the river running over the parapets of that bridge." That country was once one of the richest granaries of the Roman Empire. It now yields a scanty support for a sparse and semi-barbarous population. The whole region roundabout is treeless. The care of the national forests is a provision for future generations, for the permanence over vast areas of our country of the great industries of agriculture and mining upon which the prosperity of the country ultimately depends. The National Forestry Bureau ought to be at once organized with its director, clerks, inspectors, head overseers, assistant overseers, rangers, and field force, as recommended by the Commission appointed by the National Academy of Sciences on a Forest Policy for the United States. A good forest administration would soon come to support itself; but it should be organized in the interest of the whole country, no matter what it cost. The estimate of the cost of the organization, as made by the Forestry Commission of the Academy, was two hundred and fifty thousand dollars a year for the first five years. This is about the annual cost of the maintenance of the protected cruiser San Francisco.

The Government has carried on for many years an inquiry into the habits, feeding-grounds, and modes of breeding and migration of the fish which make an important part of human food, and inhabit the Western Atlantic and the Eastern Pacific, the Great Lakes, and the rivers and brooks of the continent. It is obvious that no power but that of the general Government can carry on effectively a research of this magnitude, covering such enormous areas, and dealing with such a variety of creatures. The waters of the globe yield food of great variety and great value to mankind; but the habits and conditions of breeding of fish and shell-fish have remained until this century almost unknown, and indeed are still

wrapped in much mystery. Yet questions are constantly arising as to possible diminution of this important food-supply, and as to the effects on the permanence of the supply of new methods of catching fish. These great questions are legitimate objects of study by the Government; but it is obvious that such researches require expensive outfit, long time, and highly-trained observers. When to these researches is added the actual breeding of young fish in great quantities for the stocking of rivers, ponds, and brooks, it becomes apparent that the field of labor is simply enormous, and that the economic interests involved are vast and permanent. Now, in this great enterprise the expenditures of the Government during the five years 1893–1897 have been $360,000 a year, which is less than the annual cost of maintaining one of our battleships.

One other mode of beneficent expenditure the United States Government has maintained for a generation, namely, the annual appropriation of money for certain colleges of agriculture and mechanic arts, which were founded under the Act of 1862. In aid of these colleges the Government appropriated in 1897 a million of dollars. It is hard to see why the Government aid should be limited to this particular sort of instruction, to which only a very small percentage of the youth of our country can possibly resort; but if the Government is going to aid exclusively the colleges of agriculture and the mechanic arts, what a pittance is one million a year! Can any of us see with satisfaction our Government spend no more on the annual support of education in agriculture and the mechanic arts throughout the country than on the annual maintenance of three battleships in time of peace?

In instituting these comparisons between military and naval expenditure on the one hand, and expenditure for purely beneficent objects, such as the advancement of science, the development of technical skill, the saving of life, the improvement of industries, and the support of education, on the other, I have no intention of even suggesting that the expenditures on military and naval preparation should be diminished, much less stopped as Charles Sumner proposed. The short war with Spain has taught us the immeasurable value of the Regular Army and Navy, and has justified the expenditure of all the money they have ever cost. As war becomes more and more a matter of science, — chemical, physical, biological, and fiscal, and of highly-trained skill on the part of all who direct or operate the complicated machinery of war, it is manifest

that it is the duty of the United States to build and maintain the most perfect instruments and appliances of war that the utmost skill of our engineers and mechanics can produce, and to keep in training adequate bodies of men to use effectively this elaborate machinery. But is it not equally clear that the nation which can afford to make this expenditure can afford to make much freer expenditures than our nation has ever made on the wholly beneficent agencies of the Government, which save life, increase food and ore production, avert evils, facilitate transportation, promote industries and commerce, and foster education? If the self-respect of the nation were habitually increased by the visible achievements of the Government in peace, there would be less chance of the people's being tempted to war by the desire to see the power of the Government exhibited. If the Government habitually displayed a great beneficent power, a power exerted primarily for the good of its own citizens, but secondarily for the good of mankind, and which, in order to its full effects, called for the permanent maintenance of large bodies of disciplined and devoted servants of an excellence comparable with that of the Regular Army and Navy, would there not be solid grounds for pride and satisfaction in our Government which would tend to defend us from seeking that pride and satisfaction in military glory?

After everything possible has been said in favor of martial virtues and achievements, whenever our people really take up the question how best to win glory, honor, and love for free institutions in general, and the American Republic in particular, whether in our own eyes or in the eyes of other nations and later times, they will come to the conclusion that more glory, honor, and love are to be won by national justice, sincerity, patience in failure, and generosity in success, than by national impatience, combativeness, and successful self-seeking; and glory, honor, and love more by as much as the virtues and ideals of civilized man excel those of barbarous man.

EXECUTIVE PROCEEDINGS.

REPORT OF THE GENERAL SECRETARY.

THE Forty-seventh Meeting and Fiftieth Anniversary of the American Association for the Advancement of Science was called to order on Monday, August 22, 1897, at 10 o'clock, A. M., in Huntington Hall of the Massachusetts Institute of Technology, Boston, Mass., by Professor WOLCOTT GIBBS, of Newport, R. I., the retiring President. After prayer by the Right Reverend WILLIAM LAWRENCE, Bishop of Massachusetts, the retiring President introduced the President of the meeting, Professor FREDERICK W. PUTNAM, of Harvard University.

President PUTNAM on taking the chair expressed his gratification at taking it from a founder of the Association and acknowledged the great honor which had been conferred upon him by the Association. He then introduced His Excellency, ROGER WOLCOTT, Governor of Massachusetts, who welcomed the Association. He said:

It is with far more than ordinary pleasure that, on behalf of the Commonwealth, I extend a most cordial greeting to those who are present here to-day. The American Association for the Advancement of Science on this, its fiftieth anniversary, has done well to return to the soil of the Commonwealth which was the place of its birth and which gave to it its charter. I welcome you all—those who come from the many cities of our country and those who come from foreign lands. The Commonwealth is honored by your presence. As seekers after truth, you have devoted your lives to following the footsteps of Science, whether her majestic way is across continents, following the pathway of stars, or whether she delights to follow the minutest particles revealed by microscopic research.

Governor WOLCOTT then described at length in an eloquent and scholarly manner the many benefits coming to the world by the researches of men of science, and, continuing, compared them to the men who have erected lighthouses on the coast, guiding and directing the course of those who will come after them. He spoke of the infinite possibilities of science, and said that, in spite of the great advances that have been made, little is known to-day of the mysteries of nature, so elusive is its touch.

It is for you, he said, little by little as the years and the centuries go on, with faithful and painstaking search, to learn a little more of that great ocean of truth and to launch your barks a little farther on the sea of science, and to know more about the stars, the plants, the pebbles and the

shells. The truth is that science is still sweeping beyond you and is beckoning you to follow her. Science would be less worthy of regard if its benefits should be limited to any class, but it is open to all.

It is as men of science that the Commonwealth welcomes you to-day. May you bear away from this meeting pleasant memories of the State, rich in the valor and achievements of her sons. And may you leave behind you that inspiration which is fostered and cherished by men who are brought together to compare notes and clasp hands and carry back memories of this meeting. The Commonwealth greets you and expresses to you her recognition, and bids you welcome to the old Bay State.

The PRESIDENT next introduced His Honor JOSIAH QUINCY, Mayor of Boston, who joined in the Governor's welcome, speaking as follows:

We are grateful to you for giving us an opportunity of seeing you here, that we may listen to your deliberations and exercises, and for having the privilege of entertaining you in some measure. I may say in truth that there is no other city in this country which would appreciate more highly the privilege of having this anniversary meeting held within its borders than the city of Boston. I think I may claim with truth that in no city is science held in higher esteem, in no city are the great work of science and the widespread beneficence of its results more recognized than in the city of Boston. There is no organization to which we would more gladly open our doors than to your American Association for the Advancement of Science. Your work has a very direct relation to the work in which the people of the city of Boston are engaged, in their corporate capacity, and the work which their municipal government is trying to prepare for them. As I regard it, the work of good municipal government is the task of securing the practical application of the principles of science to the great fund of knowledge which has been won for us by science. I am continually impressed in my practical relation to the work of this great city with the vital relation which science bears to that work. More efficient government is to be sought along the lines of affairs which fall within the scope of our municipal government, and this is to be won for us by the investigators who have increased their knowledge of science within the last fifty years.

I trust and believe that this interesting occasion may do something for us as well as something for the American Association for the Advancement of Science, and I hope that among other benefits it may give to our citizens as a whole a greater appreciation of what science is and of what science does; and not only of what science does in the abstract or in the way of theoretical interest alone, but demonstrate the value of science as the handmaid of civilization which enlarges the views of mankind and lifts society up to the highest plane of thought. If the first result is economy in its relation to finance, if its first effect is to enlarge our minds and give us a wider power of interpretation of nature, to harness more effectively the forces of the great world and to save labor, it has no less service to render in the moral sphere. First are to be considered

the economical possibilities, which mean easier living with infinite improvements in the art of living, and after it come the advancement of all humanity and the more complete progress which must accompany social uplifting. We are endeavoring in a partial and incomplete way to apply some of the principles of science to the practical benefits of the one-half million people within the limits of this municipality. I am proud to say that we give a high place in everyday work to men of science who are giving technical application to the principles which have come to light through the investigations of abstract science. Work in the future will demand a fuller employment of men of science. I am proud to say that we are commanding the interest and the services and the hearty coöperation, without price and without regard, of men who are endeavoring to give in some measure a practical social science, and, while this may be a far less exact science that many others, I firmly believe that there is a social science and a political science, and that the domains which come within its knowledge are constantly widening, both as regards the body social and its evolution and the body politic and how to secure its best application. I heartily congratulate the American Association and welcome it back, after fifty years, to the scene of its birth, and extend thanks and welcome, on behalf of the city of Boston, to each and every one of its members, in view of this meeting here and the work which the Association is yet to do in the first half of the century to come.

President JAMES M. CRAFTS of the Massachusetts Institute of Technology was then introduced and delivered the following address:

To the greetings which have been so cordially offered to you in behalf of the state and of the city it is my privilege to add a few words of welcome from the Institute, which is honored by your visit to-day.

You have not often favored us with your company, for you waited until the twenty-ninth year of the foundation of the Association before you held your first Boston meeting. Now twenty-one more years have elapsed before you visit us again, and we are pleased that it should be for the purpose of holding your semi-centennial anniversary in this city.

If your visits, like those of angels, are rare, they are all the more highly prized, and many of us feel that we must make the most of this one, because we may not, in the course of nature, hope to live to see you with us again, if your orbit is fixed by the intervals of your past appearances. While saying that you waited twenty-nine years before making your first call upon us, it should be added that long ago, in 1849, the second meeting of the Association was held in Cambridge, and that the Bostonians had the advantage of participating in that meeting in the same way that now our good neighbors of Cambridge and of other near places join in welcoming you to this one.

At the date of the Cambridge meeting Harvard College was two hundred and thirteen years old, and the institutions of Boston and the neighborhood which now have the honor of greeting you were unborn and unthought of. Since that time Tufts College, Boston College, the Mas-

sachusetts Institute of Technology, Boston University and Wellesley naming them in the order of their foundation, have grown up like young plants, and, notwithstanding the rapid increase of Harvard University, they surpass it at present in the number of their students. The total adds up to nearly 8,000 students and more than 800 teachers, about the same number as at the University of Paris.

It would be a poor greeting to deluge you with statistics, but I trust you will pardon these few, which are meant to show that since you have visited us we have been diligently occupied in preparing a fitting audience to meet your second, or rather your third coming; for the teachers and students in these colleges have come in larger part from New England, and particularly from this neighborhood, and, their education finished, they have remained in their New England homes, and whatever has clung to them of their college life is potent in shaping the modes of thought of their community. This center of education, then, is one of the largest in the world, and it is eager to give you the kind of reception you will most prize, those tokens of sympathy, of respect and of appreciation which spring from knowledge; for educated men know that men of science are discovering those truths which form the newest and truest part of their education. Sometimes the surroundings of a scientific man impress upon him a sense of isolation. He is asked: What good are such things? when he has discovered a new formula or a new exhibition of energy or a new substance, or has dissected the nervous system of a worm. He has no leisure to answer such questions, but he works on contented if he is not disturbed, and expects little applause unless he turns out a salable product. Let him, however, come in contact with those whom modern methods have trained to some knowledge of science, and he immediately finds ready support and sympathy and some share of the enthusiasm which he feels himself.

It is now as much a part of a good education to know something of scientific facts as it is to know the causes which led to the fall of the Roman Empire. I do not mean that we make scientific men of the great body of our students any more than the study of history transforms scholars to statesmen. When freshmen enter our laboratories we have small hope that they will make original discoveries, but we are well content if, while fitting themselves for some practical occupations, they learn the great lesson that new truths can only be found out by observation and experiment, and if they learn habits of honesty of thought by dealings with nature, which never lies. These men form the great public who have come forward so willingly and so generously to lend a helping hand to science. Perhaps in their college life their unskilful experiments have taught them to admire the skill which has made yours successful; their attempts at observation have given them some idea of the acuteness of powers which can be acquired by long years of faithful training.

Those who have pleasure in scientific occupations are ready sympathizers, and know something of the joy which a discovery brings with it even if there is no money in it. As to those among us—and there are many who have devoted their lives to scientific work — I have no need to as-

sure you of their hearty welcome, of their desire to meet you in this congress, and to listen to the news of your latest achievements and perhaps to tell you some of theirs. Your connection with our colleges is very direct, for many of the members of this Association have taken a leading part in this work of education, and in this neighborhood we have been fortunate in our teachers. Scholars who have walked the field with Gray and Agassiz, who have learned their mathematics from Peirce, their anatomy from Wyman, or their chemistry from your retiring president, might look the world over without finding leaders better fitted to guide them to the innermost chambers of scientific knowledge. In this place it is most fitting to mention the chairman of the first meeting of this Association, William B. Rogers, who was a born educator. He loved science for its own sake, and he had a patriotic desire to see his country call upon science to aid in its material progress. His efforts began so far back as 1828, when he thought it useful to lecture to the American people in Baltimore upon the advantages which he hoped would be derived from building railroads in this country. He demonstrated the known principles of railroad building, and showed that traction upon smooth iron rails was possible. Afterwards, when he came to Boston, his thoughts were full of a project for interesting the community in providing means for the education of men to direct our growing industries. This Institute was founded, and this building erected because Rogers knew how to make science popular; his contagious enthusiasm inspired many co-workers who have not yet ceased in their task. With great simplicity of character he united an ardent imagination which gave a singular fascination to his public exposition of scientific truths. You perhaps know him best through his earliest endeavors for the foundation of the Geological Society, and you know that, later, just fifty years ago, he contributed with all his heart to the formation of this Association. His later years were devoted to the Institute which he built up, and which now, largely grown from small beginnings, has the honor of welcoming you this day; and it was on this stage that he fell, an unfinished sentence on his lips, giving his life to the cause which overtaxed his strength.

The memories which attach themselves to this place have led me to speak thus at length on this fiftieth anniversary of one who was a principal founder of our Association, and yet other memories crowd into this hall.

The Lowell lectures have been held here for many years, and to your Association belong many of the eminent men who have stood upon this platform, and who have done much to make the Boston public no stranger to scientific assemblies. Some sixty-five years ago the strongest interest in lectures was excited in New England by the qualities of certain lecturers. The eloquence of Edward Everett, the character, the new doctrines and the fascinating delivery of Emerson made men feel that book knowledge was of little worth, and that the living voice was the true means of communication with man. It seems like the difference between reading testimony or hearing a witness. Under these impressions John Lowell, Jr., a young man of thirty-four, after the misfortune of losing wife and

children, made a will by which he devoted half his fortunes to founding courses of free lectures. His death happened soon after in a foreign land, and the fund came into the hands of his cousin, John Amory Lowell, who made its care and administration the chief occupation of his life and has been succeeded in the charge by his son. In these hands the Lowell lectures have grown to be the largest enterprise of the kind in the world. The fund suffices to maintain five hundred to six hundred free lectures yearly, and to offer inducements to the most distinguished men in all English-speaking lands to come to speak to Boston audiences. We owe to this enterprise the visit of many a man of science to this country, and, in one notable case, a permanent settlement. You all know that Louis Agassiz was called to the United States to deliver a course of Lowell lectures, and that he became as good a citizen as he was a savant.

As you will see by the guide-book prepared for the use of the Association, great libraries and museums have kept pace with the intellectual and material growth of the community, and the needs of science are represented in them as well as those of art and literature. Our museums depend more than other institutions upon popular appreciation for their support, and calls upon public liberality must be seconded by a presentation in some striking and evident way of the aims and scope of the work which the collections illustrate. I cannot help thinking that your presence and your discussions, and the effort which you make to come each year, often from great distances, contribute notably to keep the cause of science before us, and that you aid in its task each community you visit.

As you watch the motions of the stars, or make experiments in laboratories, or observations in the fields, or build bridges, as you seek to cure disease and alleviate pain, or reduce the actions of mankind to fixed laws, you doubtless have sometimes in view your summer meeting, and look forward to talking over with each other your discoveries and your difficulties, and as you are willing to do this in public, the whole community as your place of meeting comes to a better knowledge of the beautiful, earnest, skilful effort which makes up the life of a man of science. It is not a congress to adjust conflicting interests or for displays of oratory which lead to no conclusions. We get to know the man himself, and I think he would be even more popular than he is at present if we could invent some suitable name for him. Scientist is a most ill-sounding word. The French term savant — a knowing one — might provoke a smile, when contrasted with the simplicity of character of many men of science, but perhaps after all this is your best title to fame. Your discussions do not often degenerate into disputes, because for the first time in the history of the world methods of work have been found so sure that the results can be accepted almost without discussion. Even the geologists come to an eventual agreement about their theories, and the account which Bret Harte has given about their meetings must not be taken too literally. You are good witnesses and generally agree upon your facts, and when facts lead to differences of interpretation the single-minded desire to reach the truth brings you into accord at last.

We have good reason for welcoming you among us, and although you

do not come as missionaries we shall find ourselves the better for your coming.

M. CHARNAY, the official delegate of the French government, was introduced to the meeting by PRESIDENT PUTNAM, and delivered in the French language an address which may be rendered into English as follows:

GENTLEMEN:—

Allow me to tell you how happy I am to have been selected by the French government to be its representative on this great occasion. Let me also state that it is not on account of my humble merit that this honor has devolved upon me, but rather because of my frequent sojourns in your great and beautiful country; and it is also, perhaps, because I am believed to be half American, that I have been chosen as delegate by my government.

My first journey here dates back to 1854; it is, as you see, forty-four years ago, and I am almost celebrating a jubilee.

Since that time, I have often been here, and I have noted each time the progress realized by your country in all branches of human knowledge, for it is not in sciences alone that the American genius has developed; arts have followed the same rate of progress: your designers, your artists come to Paris and rival our own in winning prizes and honors.

Your engineers and your architects are the first in the world, and no nation has given in such a short time such proof of a high vitality.

You may have been told, gentlemen, that our sympathies for the American people had lessened: this is absolutely false, and only the deliberate hostility of a foreign press could so misrepresent us.

It is hard to believe that certain foreign papers have dared to say that the French people committed all assaults against Americans.

These absurd statements have been reproduced by some American papers with the following additions: "Who will wonder at our protestations against the attacks of the French, when we hear that American women have been insulted in hotels and in the streets; when we say that young American girls have been obliged to escape to London from such annoyance, etc., etc."

I could quote more just as unjust passages, but I leave it to you to see the absurdity of such odious lies, which are unworthy of discussion, foreign as they are to the French character, and which American good sense must at once dismiss as untrue.

Most of our leading papers have protested with indignation against these infamous statements, and I, in my turn, deny these calumnies and allegations.

Yes, gentlemen, I affirm openly that all this is false. I affirm that the old friendship which binds our respective nations is as strong as ever, and that *we* rely upon yours as much as we ever did.

In acknowledgment of these addresses PRESIDENT PUTNAM thanked the Local Committee for the hearty welcome extended to the Association by the Governor of the State, the Mayor of the City and the President of

the Institute of Technology. In their several addresses of welcome, they had so well expressed and so fully appreciated the objects of the Association that it was not necessary for him to say what the Association hoped to do for the State and the City by holding its meeting in this acknowledged centre of culture. The President then recalled the Boston meeting of eighteen years ago and the prominent men who took part at that time. He mentioned the surviving founders of the Association, — Gibbs, Dixwell, Abbott, West and Hubbard; and the surviving founder of the parent association, — Boyé. He feelingly alluded to the recent death of James Hall, who had been looking forward to attending this anniversary meeting, and who at the time of his death was the oldest living President of the Association. He referred to the Massachusetts men who had held the office of president, and announced that in memory of three who were no longer on the list of living presidents, the Council had made Mrs. Wm. B. Rogers, Mrs. Louis Agassiz and Mrs. Asa Gray Honorary Associates. The President expressed his feelings on giving up the office of Permanent Secretary which he had held for twenty-five years, and gave a brief summary of his forty-two years' connection with the Association. He had joined the Association, he said, at the age of seventeen, and realizing from his own experience the great value of membership in the Association, he urged all young men and women to become members and receive the benefit of contact with the great minds in science.

In the course of his remarks, the President called attention to the tendency to specialize in all departments of science which has led to the formation of many special societies. This specialization, he said, should not prevent the attendance of members of these societies at the meetings of the Associations, where they would be benefited by contact with their fellow scientists and would aid in the advancement of science by diffusing their special knowledge.

The GENERAL SECRETARY read the following cable message from the Russian Geological Congress at St. Petersburg:

St. Petersburg, Aug. 19, 1898.

BOSTON INST. TECH.

PROFESSOR TYLER:

Russian Geological Committee sends to American Association for Advancement of Science respectful congratulations and best wishes.

KARPINSKY.

He also read a resolution of the COUNCIL that members of affiliated societies and residents of Boston and vicinity should be admitted to associate membership for this meeting on the payment of three dollars after regular nomination and election.

PRESIDENT PUTNAM then introduced the new Permanent Secretary, Dr. LELAND O. HOWARD, as one who would give the Association faithful and efficient service, and he asked for him the same appreciation that had been accorded to himself. Dr. HOWARD made the customary announcements regarding the organization of Sections.

In the unavoidable absence of the LOCAL SECRETARY, the Chairman of the Executive Committee of the Local Committee, Professor SEDGWICK, announced the arrangements that had been made for the accommodation and entertainment of the members.

After agreement on the hours of meeting as printed in the preliminary programme the general session was declared adjourned.

The closing general session was held in Huntington Hall, at 10 A.M., Saturday, August 27, PRESIDENT PUTNAM in the chair.

The President called upon Mr. W J MCGEE as chairman of a committee on resolutions of thanks. Mr. MCGEE presented the following:

WHEREAS, the American Association for the Advancement of Science has, after a half century of wandering, returned to its birthplace to celebrate its fiftieth anniversary, and to signalize that unprecedented development of science which it has ever striven to promote:

WHEREAS, this Jubilee Meeting in Boston has been eminently successful in bringing out the results of scientific research, and eminently agreeable in the personal associations formed and renewed; and

WHEREAS, the success and pleasure of the meeting have been due to the unbounded hospitality and untiring courtesy of the people and institutions of this American centre of culture: Therefore be it

Resolved, that the visiting members and associates of the American Association for the Advancement of Science do hereby tender cordial thanks and hearty assurances of pleasant memories

To the Commonwealth of Massachusetts and His Excellency, Governor Roger Wolcott, for most courteous welcome to the Old Bay State;

To the municipality of Boston and His Honor, Mayor Josiah Quincy, for a welcome to the city, and for facilities and courtesies innumerable;

To the Salem Committee and its Chairman, Honorable Robert S. Rantoul, for the freedom of the ancient city of Salem;

To the Massachusetts Institute of Technology for our temporary home, and to its distinguished President, Professor James M. Crafts, for his initial welcome and sustained courtesies;

To the citizens of Cambridge, and especially the corporation of old Harvard, for a welcome to the neighboring city and the halls of the University, and to Professor Charles W. Eliot for his eloquent and inspiring address;

To the trustees and officers of the Boston Museum of Fine Arts and of the Boston Public Library for receptions within the stately walls of these noble institutions;

To the Governor and Mrs. Roger Wolcott, to Mrs. W. B. Rogers, to Mrs. John C. Phillips, and to Mr. and Mrs. John L. Gardner for receptions in their hospitable homes;

To the local committee in general, and especially to Professors William T. Sedgwick and H. W. Tyler for elaborate arrangements for the meeting, and for untiring assiduity and courtesy in carrying them out;

To the Technology Club for the working room for the council, and for various facilities and favors; and finally

To the good people of Boston, all and several, who have contributed to make our stay pleasant and profitable—their name is legion, and their favor is a delight.

The following resolution was then introduced by Dr. H. C. HOVEY, on behalf of the American Association for the Advancement of Science and the Geological Society of America, jointly:

WHEREAS, special acknowledgments are due to the Boston Society of Natural History, first, for initiating the movement for the Jubilee Meeting in Boston, and afterward for making material provision for the accommodation of the Association and the affiliated societies, therefore be it

Resolved, that the hearty thanks of the American Association for the Advancement of Science and the Geological Society of America be extended to the Boston Society of Natural History and its eminent President, Professor Charles S. Minot.

On behalf of the American Association for the Advancement of Science and the National Geographic Society, jointly, W J MCGEE, as Vice President of the latter society, submitted the following supplementary resolution:

WHEREAS. the members of the several affiliated societies who have enjoyed the pleasure of this Jubilee Meeting of American Science are under special obligations to the Appalachian Mountain Club for hospitalities beginning weeks before the meeting and promised to continue for days beyond its close, therefore be it

Resolved, that the American Association for the Advancement of Science, the National Geographic Society, and all other affiliated societies represented in this assemblage do hereby tender special thanks and assurances of grateful remembrances to the Appalachian Mountain Club for the railway facilities it has provided, for the excursions it has arranged, and for the many courtesies extended by its officers and members.

The several resolutions were seconded collectively by past president Dr. DANIEL G. BRINTON, speaking on behalf of the United States and Canada; Professor DÉSIRÉ CHARNAY of Paris, by Dr. BENJAMIN HOWARD of London on behalf of the invited guests, and by Dr. H. C. HOVEY.

The following letter was also read:

Train from Boston to New York City,
Aug. 24, 1898.

MY DEAR PROFESSOR MENDENHALL:

There is a most important message which I was going to ask your favor to convey to the American Association for the Advancement of Science before leaving Boston, but in the flying moments which we passed so pleasantly it escaped my recollections and that is to return my most cordial thanks to the Association for the honour they gave me in inviting me as one of their guests to their fiftieth anniversary meeting. It will be most gratifying to my government and people to know that we were represented on this memorable occasion. I only regret that I could not stay longer, but if you will kindly explain the circumstances of my visit to Boston, I hope your Association will be good enough to forgive me in taking leave in this unceremonious manner; be so

good as to forward my best wishes for the advancement of your scientific works with which you have already glorified your country, and for happy returns of many such occasions.

Yours ever,

A. TANAKADATE.

The appreciative expressions were gracefully acknowledged by Professor WILLIAM T. SEDGWICK, Chairman of the executive branch of the Local Committee; he then presented Dr. EDWARD EVERETT HALE, who made further responses on behalf of the people of the city and state.

It was announced by the secretary of the COUNCIL that the following alterations in the Constitution of the Association had been recommended by the COUNCIL, and would be put to vote in general session next year:

ARTICLE 3. Add:

"Associates for any single meeting shall be admitted on the payment of three dollars; such associates to have all the privileges of the meeting, except reading papers and voting.

"Members of scientific societies whose meetings are contemporaneous with or immediately subsequent to that of the Association and which are recognized by vote of the Council as 'Affiliated Societies,' may become associate members for that meeting on the payment of three dollars. They shall be entitled to all the privileges of membership except voting or appointment to office, but their names shall not appear, in the list of members printed in the annual report.

"Any member or fellow of any national scientific or educational institution, or of any society or academy of science, of any country not in America, who may be present at any meeting of the Association, shall, on presenting the proper credentials, be enrolled without fee as a Foreign Associate and shall be entitled to all the privileges of the meeting except voting on matters of business."

ART. 9. So as to read:

"The officers of the Association shall be elected by ballot by the *General* Committee," etc., as at present.

ART. 11. Add to the phrase:

"At such time as the Council shall determine," the following: "at the meeting subsequent to that at which he presides."

ART. 17. So as to read:

"In case of a vacancy in the office of President the Senior Vice President shall preside, as provided in Article 10, until the General Committee can be assembled and the vacancy filled by election. Vacancies in the offices of Vice-President, Permanent Secretary, Secretary of the Council, Secretaries of the Sections and Treasurer, shall be filled by the Council by ballot."

ART. 18. Insert the words:

"Two meetings" instead of "meeting" in second line.

ART. 19. So as to read:

"The General Committee shall consist of the Council and one member or Fellow elected by each of the sections, who shall serve until their successors are elected. It shall be the duty of the Committee, etc., etc.,"—as at present—*except* the word "General" to be substituted for "nominating" in the last sentence. Substitute "the Sectional Committee" for the last two lines beginning "a Sub-Committee."

ART. 20. Substitute:

"General" for "Nominating" in third line.

ART. 23. Insert:

"Members or" between "three" and "fellows" in second line. Add "The Sectional Committee may invite distinguished Foreign associates present at any meeting to serve as Honorary members of said Committee."

ART. 25. So as to read:

"No paper shall be read in any section or sub-section until it has been placed on the programme of the day by the Sectional Committee."

ART. 26. Substitute:

"Committee" for "Council" in the fifth line. Add "The Sectional Committee shall also make nominations to the General Committee for Vice-President and Secretary of their respective sections as provided for in Article 19." Also change "returned" to "referred" in next to last line."

ART. 28. Insert:

"Secretary of the proper section or to the" before "Permanent" in the first line, and substitute "considered by a" for "referred by the Council to the" in the last sentence.

ART. 29. So as to read:

"If the author of any paper be not ready when called upon in the regular order of the official program, the title may be dropped to the bottom of the list."

It was also announced that the COUNCIL had made a grant of $50 to the Committee on Standards of Measurement for work being carried on by Prof. H. S. CARHART, and a grant of $50 to the Committee on Ethnology of the White Race in America for instruments to be constructed by Prof JAMES MCKEEN CATTELL. Other applications for grants had been laid on the table on the ground that they did not fulfil the conditions of a resolution passed by the COUNCIL, which was as follows: —

Resolved that grants be awarded under the following conditions: —

(1) That a formal request be received for such grants.

(2) That a grant be awarded for a specific investigation only.

(3) That a report be submitted to the Association describing the results of such investigation.

The attention of the members was called to the sixteenth annual report of the Committee on Indexing Chemical Literature, which had been distributed in the Section of Chemistry, and to the report of the Committee on Standards of Measurement, which was ready for distribution in printed form.

[These reports are printed elsewhere in the volume.]

The following report of the Standing Committee on Library was presented:

The Standing Committee of the Association on Library would respectfully present the following report:

The Library, now placed in the charge of the University of Cincinnati, has been under careful supervision during the past year and has been accessible daily for purposes of consultation.

The entire collection is now classified on the Dewey or decimal system, and all accessions during the past year have been properly classified and located.

Arrangements are now being made by the University authorities to complete the binding of the collection, and this will be followed by the completion of the catalogue.

Until this is finished, no satisfactory attempt can be made to provide for the circulation of the volumes in the Library at a distance.

The shelving and arrangement of the books, the facilities for direct consultation and the protection from danger by fire, are all that could be desired.

Respectfully submitted,

ALFRED SPRINGER, *Chairman*,
T. H. NORTON,
THOMAS FRENCH, JR,
W. L. DUDLEY,
AMOS W. BUTLER.

The following new committees were announced:

1. To coöperate with the National Forestry Association of Germany: W. H. BREWER, *Chairman;* GIFFORD PINCHOT, and ARNOLD HAGUE.

2. On the increase of the efficiency of the U. S. Naval Observatory: E. C. PICKERING, *Chairman;* T. C. MENDENHALL, R. S. WOODWARD.

It was announced that the COUNCIL had passed a resolution encouraging the Sectional Committees to procure reports on recent progress from competent scientists; that the COUNCIL had voted that at future meetings the General Secretary, the Secretary of the Council, and the Secretaries of Sections shall each receive the sum of twenty dollars towards their personal expenses, provided that all the secretaries reside in the same house during the meeting; that the COUNCIL had authorized Section H to hold a winter meeting; and that the next meeting of the Association would be held at Columbus, Ohio, August 19, 1899, under the presidency of Prof. EDWARD ORTON.

The COUNCIL had considered invitations from Philadelphia, Jacksonville, Chicago, Niagara Falls and Denver, those from Philadelphia and Jacksonville being especially urgent and cordial. The invitations from Columbus, Ohio, the city chosen, were as follows:

Ohio State University,
Columbus, August 8, 1898.

PROF. F. W. PUTNAM,
PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

DEAR SIR:—

The Trustees and Faculty of the Ohio State University cordially invite the American Association for the Advancement of Science to hold its meeting of 1899 in Columbus, and to use the buildings and grounds of the University on that occasion.

Very truly yours,

ALEXIS COPE,
Secretary Board of Trustees.

State of Ohio, Executive Department,
Office of the Governor,
Columbus, August 13, 1898.

PROF. F. W. PUTNAM,
PRESIDENT AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE
BOSTON, MASS.

SIR: —

It gives me great pleasure to offer this communication as a hearty second to the invitation which will be given to the American Association for the Advancement of Science, and affiliated societies, to hold the next annual session in the city of Columbus, state of Ohio.

I beg to assure you that, speaking for the people of this city, as well as for those of the state, the presence of so distinguished a body would be greatly appreciated and that the welcome and hospitality which would meet your members would be as warm and as generous as possible. I need not remind you that Ohioans in the past and present have occupied most honorable places in the scientific world and that, therefore, it would be fitting that the Association should honor this state by meeting here. In this beautiful city, which as you know is the home of the state's chief institution for higher learning and the advancement of the arts and sciences, the members of the Association would certainly have a pleasant and profitable sojourn. I earnestly and heartily urge upon your membership the invitation that has been extended upon the part of Ohio.

Trusting that Columbus may be chosen as the place for the meeting of 1899, I am, ir, with highest respect,

Your obedient servant,

ASA S. BUSHNELL,
Governor of Ohio.

Executive Department, City of Columbus,
August 11, 1898.

PROF. F. W. PUTNAM,
PRESIDENT AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

DEAR SIR: —

In behalf of the citizens of Columbus, Ohio, I beg leave to extend to the American Association for the Advancement of Science, and affiliated societies, an invitation to hold their next annnal meeting in this city.

I assure you, if you will thus honor us, we will exhaust every effort to make the visit of the members not only pleasant, but also, so far as lay in our power, profitable.

Hoping that you may be able to give us favorable consideration, I remain,

Yours sincerely,

SAM'L L. BLACK.

The Board of Trade of Columbus, Ohio,
July 30, 1898.

TO THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

GENTLEMEN: —

The Board of Trade of the city of Columbus, Ohio, desires to extend to your most honorable body a cordial invitation to make your next meeting place with us. We feel that we have much to offer your Association in the way of centrality of location, splendid railroad facilities, unexcelled hotel accommodations, to say nothing of the hospitable character of our people, assuring you the most kindly and generous entertainment.

We shall feel complimented by your acceptance, and will do all in our power as an organization, and as individuals, to make your stay enjoyable.

Very respectfully,

JOHN Y. BASSELL,
Secretary.

Denison University, Granville, Ohio,
August 15, 1898.

PROF. N. W. LORD, COLUMBUS, OHIO.

DEAR SIR: —

I am pleased to learn that an effort is making to have the next session of the American Association for the Advancement of Science held at Columbus. I most heartily approve of such a proposition. As president of the "Ohio College Presidents' Association," I desire to extend a most cordial invitation from that body to the American Association to hold their meeting within the bounds of our State.

As president of Denison University, I extend a like cordial invitation from the trustees and faculties of the institution which I represent.

Very truly yours,

D. B. PURINTON,
President of Denison University.

Ohio State University, Department of Geology,
Columbus, August 18, 1898.

DR. L. O. HOWARD,
PERMANENT SECRETARY, A. A. A. S., BOSTON, MASS.

DEAR SIR: —

An invitation to the Association to hold its next meeting (1899) in Columbus, Ohio, will come before it through proper channels.

I shall not be present at the Boston meeting, and therefore beg the privilege of supporting our invitation, as best I may, by a brief letter.

The main points, appropriateness and accessibility of location, suitable quarters for the meetings, hotel accommodations, will have been set before the Association so fully as to need no reinforcement. I can assure you of a very cordial reception on the part of our people. The city will take pride in welcoming the Association, and will leave nothing undone on its part towards making the meeting successful.

As far as American Archæology is concerned, we can provide excursions of unrivalled interest. Ohio geology, with its paleontological and economic wealth, can be surveyed to excellent advantage from the capital of the State.

As to the weather to be expected, I may add that while "hot waves" sometimes steal in upon us, they come from other regions and go elsewhere. August in central Ohio often gives us delightful summer weather.

I hope that our invitation will be accepted, and that Columbus will be honored by being added to the list of Association cities.

Very truly yours,

EDWARD ORTON.

Columbus, August 17, 1898.

TO THE PRESIDENT AND COUNCIL OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

GENTLEMEN: —

The Ohio State Board of Agriculture extends to you a cordial invitation to hold your next annual meeting in the city of Columbus, Ohio, and will do all in its power to make your visit to this city agreeable and profitable.

Yours very truly,

W. W. MILLER,
Secretary.

The Ohio State Archæological and Historical Society,
Columbus, August 16, 1898.

PROF. F. W. PUTNAM,
PRESIDENT AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE,
BOSTON, MASS.

DEAR SIR:

At the regular monthly meeting for August, of the Trustees of The Ohio State Archaeological and Historical Society, I was instructed, as Secretary, to write you that our Society extends to your Association a most urgent and hearty invitation to hold your next annual convention, 1899, in this city, Columbus, Ohio.

Our Society will coöperate most heartily with many other institutions interested in the same purpose, to make your convention a success, and the sojourn of your members in our city pleasant and profitable.

As you are fully aware our Society is interested in the archæology of the state. That science is represented in one of the important branches of your Association. As you well know Ohio is rich in archæological research. During the stay of your Association in our city we could arrange for an excursion to Fort Ancient, which we believe would be a most delightful and interesting event for the members.

I assure you, personally, sir, that our Society will be exceedingly gratified to meet and welcome you to our city, and we trust that you will give Columbus careful consideration in the selection of the place for your next convention. I am, sir,

Your most obedient servant,
E. O. RANDALL.

Columbus Horticultural Society,
Columbus, August 12, 1898.

TO THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

GENTLEMEN: —

The Columbus Horticultural Society extends a most cordial invitation to the members of your Association to hold its next annual meeting, that of 1899, in the city of Columbus, Ohio.

We earnestly invite you to meet in our capital city, not alone because our own membership unanimously desire it, but also because the public spirited men and women of other scientific and educational institutions of Columbus and vicinity, will unite with us in doing all possible to make the meeting a signal and all-inspiring success.

Columbus has never been honored with a meeting of your Association, but if our invitation is accepted you may be assured that the very best efforts of the city will be put forth to make the occasion one of equal interest and profit.

WILLIAM R. LAZENBY,
President Columbus Horticultural Society.

Ohio Society of Surveyors and Civil Engineers,
Granville, O., August 11, 1898.

TO THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, BOSTON, MASS.

GENTLEMEN: —

The Ohio Society of Surveyors and Civil Engineers unites with other State Societies and with organizations in the city of Columbus, Ohio, in a very cordial invitation to your Association to hold your next annual session in the capital city of Ohio.

Very respectfully,
J. L. GILPATRICK,
Pres. O. S. of S. & C. E.

C. N. BROWN,
Sec'y & Treas.

Columbus, O., August 20, 1898.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The Columbus Section of the American Chemical Society cordially invite the American Association for the Advancement of Science as well as all affiliated organizations to hold their next annual meeting at Columbus, Ohio, and promise to do all in their power to make the meeting pleasant and profitable.

Very respectfully,
H. A. WEBER,
Chairman.

Columbus, O., August 10, 1898.

PROF. F. W. PUTNAM,
PRES. AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, BOSTON, MASS.

DEAR SIR:—

The Engineers' Club of Columbus sends greeting to your honorable body on the occasion of your jubilee and congratulates you on the completion of fifty years of useful and distinguished service which has contributed so greatly to the sum of human knowledge.

The full membership of this Club unite in extending their heartiest invitation to your Association to honor this city by choosing it for the place of your annual meeting in 1899.

Very sincerely,
The Engineers Club of Columbus,
JULIAN GRIGGS,
President.

Attest H. M. GATES,
Secretary.

The PERMANENT SECRETARY then read a brief account of the Boston meeting, after which the PRESIDENT formally declared the session adjourned.

JAMES MACMAHON,
General Secretary.

REPORT OF THE PERMANENT SECRETARY.

THE fiftieth anniversary meeting of the Association was in many respects the most successful in the history of the organization. In point of numbers the attendance made it the fourth meeting in the history of the Association. The scientific and other advantages of Boston, and the fact that the meeting celebrated the fiftieth anniversary of the founding of the Association, attracted members in large numbers. The total registration was 903, and almost every State in the Union was represented. The State which had the largest representation was Massachusetts, with a total of 231; New York came second, with 158, and the District of Columbia third, with 96. The other states were represented as follows: Pennsylvania by 55; Ohio by 53; Connecticut, 29; New Hampshire, 23; Michigan, 23; New Jersey, 20; Maryland, 19; Wisconsin, 18; Missouri, 17; Illinois, 15; Rhode Island, 14; Indiana, 12; Maine, 10; Virginia, 10; Iowa, 10; Vermont, 7; Minnesota, 7; North Carolina, 6; Florida, 6; Kansas, 5; Mississippi, 5; Kentucky, 5; West Virginia, 4; Georgia 4; Louisiana, 3; Tennessee, 3; Texas, 3; Colorado, 3; South Carolina, 2; Nebraska, 2; and Montana, North Dakota and California, 1 each.

The term American has always been held by the Association to include not only citizens of the United States, but of other American countries, and members from Canada have always been present at the meetings. At this session there were present sixteen members from British North America. There were representatives from other countries in attendance. The Republic of France sent an official representative, who, with his wife, appears upon the registration list of the meeting. Three persons registered from Great Britain, one from New South Wales, one from Brazil, and one from Japan.

Not alone in point of the numbers in attendance was the meeting remarkably successful. More papers than usual were read before the different sections. Considering that during the week two entire days were spent at Salem and Cambridge during which no papers were read, and that the first day, Monday, was entirely occupied by the general session and the delivery of the addresses of the Vice-Presidents, it is remarkable that the Association should have completed the reading and discussion of so many different papers. In all 443 papers were considered during the week. Of these 39 were presented to Section A; 51 to Section B; 90 to Section C; 20 to Section D; 54 to Section E and the affiliated societies which met with it; 35 to Section F; 56 to Section G; 10 to the Botanists' Club; 55 to Section H; 33 to Section I.

Eleven affiliated societies met with the Association -- a larger number than at any previous meeting, and it is worthy of note that the Geological Society of America and the National Geographic Society joined forces with Section E of the Association in the same way that the American Chemical Society has for two years past met with Section C. So also the American Folk-lore Society met on Thursday, Aug. 25, with Section H.

Of the 272 members elected since the Detroit meeting 209 have perfected their membership; also 11 of those elected at the Detroit meeting and 1 elected at the Buffalo meeting have perfected their membership; 52 have paid their arrears, and in addition 5 have paid life membership fees to be reinstated under their original date of membership and all these have been restored to the roll; 1 name was omitted from the Detroit list by mistake, but is now printed in the Boston volume; 1 more founder of the Association has been added to the list of fellows as a life fellow, making 280 names added to the roll since the Detroit volume was published.

From the Detroit list 25 names (including 2 founders and one other honorary fellow of the Association) have been transferred to the list of deceased members; 18 members and fellows have resigned, and 119 have been omitted for arrearages, making a deduction of 162 from the list.

Thirty-one members have paid fellowship fees and have been transferred to the list of fellows.

The following is a comparative statement of the roll as printed in the Buffalo and Detroit volumes and in the present volume:

	Buffalo.	Detroit.	Boston.
Living patrons, ...	2	2	2
Corresponding members, . .	1	1	1
Members, ...	991	844	949
Living honorary fellows . .	3	3	1
Fellows, ...	805	760	776
	1,802	1,610	1,729
Honorary life members (founders) included in the above .	6	6	6

The distribution of publications since the last report is as follows:

Memoir No. 1: sold, 1 copy; Proceedings, Vols. 1-45, delivered to members, 249; sold, 78; exchanges, 5; duplicate copies to members, 3; = 335. Proceedings, Vol. 46, delivered to members, 1283; sold, 27; exchanges, 247; presented, 3; = 1560. Returned by members, 2; returned by an exchange, 1. Constitution, sold, 3 copies. Subscription has been received for 1 copy of Volume 47.

No changes in the constitution were adopted at the meeting, although, as appears from the Report of the General Secretary, a number of amendments were proposed and considered by the Council, most of them being dependent upon changes adopted at the Detroit meeting. These proposed amendments will come up for consideration at the Columbus meeting.

In the last volume of the Proceedings the former Permanent Secretary, Professor Putnam, in closing his report referred to his long term of office with appreciative words for the courteous and helpful attitude of members of the Association towards himself personally and officially. Appreciation as between Professor Putnam and the Association has not, however, been one-sided, and the Association has shown so far as it has been able to do so by official action its personal regard for the former Permanent Secretary and its estimate of his ability by electing him to the highest office in its gift—that of President. The writer feels much diffidence in assuming an office which has been so long and ably filled but assures the Association of his devotion to its interests.

L. O. HOWARD,
Permanent Secretary.

Nov. 3, 1898.

REPORT OF THE TREASURER.

In compliance with article 15 of the Constitution, I have the honor to submit the following report showing receipts, disbursement and disposition of funds of the Association for the year ending June 30, 1898.

Receipts have come into the keeping of the Treasurer from two different sources, namely: first, from contributions to the Research Fund of the Association and, secondly, from interest on funds of the Association deposited in Savings Banks. The aggregate of these receipts for the year is $231.01.

Disbursements made in accordance with the directions of the Council amount to $100 00

The details of receipts, disbursement and disposition of funds are shown in the following itemized statement:

THE TREASURER IN ACCOUNT WITH
THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

DR.

1898.			
June 30.	To balance from last account . .		$5,598 36
	To amount received as contributions to Research Fund.		27 00
	To amount received as interest on funds of the Association deposited in Savings banks as follows:		
	From Cambridge Savings Bank, Cambridge, Mass., . . .	$12 86	
	From Emigrant Industrial Savings Bank, New York, N. Y., . .	48 18	
	From Institution for the Savings of Merchants' Clerks, New York, N. Y.	73 03	
	From Metropolitan Savings Bank, New York, N. Y. . . .	69 94	
			204 01
	Total		$5,829 37

CR.

1897.			
Aug. 23.	By cash paid as grant to Marine Biological Laboratory, Woods Holl, Mass.		$100 00
	By cash on deposit in banks as follows:		
	In Cambridge Savings Bank, Cambridge, Mass.	331 56	
	In Emigrant Industrial Savings Bank, New York, N. Y. . .	1,413 25	
	In Institution for the Savings of Merchant's Clerks, New York, N. Y.	2,142 56	
	In Metropolitan Savings Bank, New York, N. Y.	1,801 80	
	In Fifth Avenue Bank, New York, N. Y.	40 20	
			$5,729 37
	Total		$5,829 37

R. S. WOODWARD, *Treasurer.*

NEW YORK, N. Y., AUGUST 18, 1898.

I have examined the foregoing account and certify that it is correctly cast and properly vouched.

EMORY MCCLINTOCK, *Auditor.*

F. W. PUTNAM, PERMANENT SECRETARY

THE AMERICAN ASSOCIATION FOR

For the year ending

Dr.		
To balance from last account		$2,670 23
Admission fees previous to Detroit Meeting .	50 00	
" " for Detroit Meeting . . .	480 00	
Assessments " " . . .	1,395 00	
" previous " " . . .	1,635 00	
Associate fees " " . . .	54 00	
Fellowship "	86 00	
		3,700 00
Publications and binding	98 11	
Miscellaneous receipts	4 93	
Gift of F. C. Robinson	17 00	
		120 00
		$6,490 27

I certify that I have examined this account

CAMBRIDGE, MASS., MAY 9, 1898.

IN ACCOUNT WITH

THE ADVANCEMENT OF SCIENCE. Cr.

December 31, 1897.

By Publications.		
On account of printing 2500 copies Proceedings Vol. 45, Buffalo volume	$931 19	
For extra copies addresses, reports, etc., Vol. 45	133 91	
For extra copies addresses, reports, etc., Vol. 46	98 75	
For illustration for Detroit volume	1 00	
" binding Proceedings	25 82	
" back volumes bought	8 00	
		1,198 67
By Expenses Detroit Meeting.		
General expenses	387 72	
Printing Detroit pamphlet	48 35	
Of Sections, including Advance Programmes	127 22	
		563 29
By General Office Expenses.		
Rent of office one year	132 00	
Moving to new office	22 97	
Petty office expenses and stationery	31 35	
Printing circulars, notices, etc.	53 20	
Extra clerical labor and typewriting	33 38	
Telegraph and telephone	18 54	
Postage Guide, $2.00; Post Office box, $6.00	8 00	
Postage, $183 47; Express, $194.83	378 30	
		677 74
By Salaries.		
Permanent Secretary	1,250 00	
Assistant "	720 00	
Janitor	100 00	
		2,070 00
By cash paid Treasurer		417 00
By Balance to new account		1,563 57
		$6,490 27

and that it is correctly cast and properly vouched for.

S. C. CHANDLER, *Auditor.*

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